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The Ideals of Inquiry

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G. E. R. LLOYD

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Introduction

THE ambition of some of those who made rather amazing claims to sagehood in antiquity is well illustrated with a quotation from the *Bṛhadāraṇyaka Upaniṣad*, generally thought to date to the 6th century BCE, where three sages are debating some arcane problems, for example about what holds the world together. One sage, named Uddālaka, asks another, Patañcala Kāpya, whether he knows the string on which this world and the next, as well as all beings, are strung together. To which Kāpya replies: 'That, my lord, I do not know.' But then another sage, Yājñavalkya, says he knows. However, Uddālaka does not allow him to get away with that bare assertion. 'Of course, anyone can say "I know, I know": tell us what precisely you know.'[1]

I shall not, of course, myself be attempting in this book to tackle such abstruse problems: rather, my principal agenda concerns three interrelated questions to do with ancient investigation, namely how it was thought it should be pursued, what was assumed about what there is to be investigated, and what investigation was thought to be good for. We think we have robust answers to those three questions where the natural world is concerned, for does not science have a scientific method, clear ideas of what there is to study, from DNA to fundamental particles to distant galaxies, and clear aims, not just understanding, but also an ambition, even a responsibility, to provide the key to the ongoing material progress that modern societies have come to demand? But what answers were given long before science as we know it today existed—in ancient societies, for instance?

I aim to explore how inquiry itself came to be the focus of attention, analysis, and controversy at the very outset of ancient investigations into the physical world. I am concerned to find out what ancient

[1] *Bṛhadāraṇyaka Upaniṣad* 3.7, Olivelle 1996: 41. I shall be returning to this debate in Chapter 2.

investigators themselves were after, not with the aim of judging how close they came to our science—for they were not to know that they should have been trying to anticipate us—but rather to assess their endeavours in *their* terms. We are all aware, more or less, of what *our* science owes to our institutions, values, and aims: but what did their inquiries owe to theirs? We shall see how ideals for inquiry were forged not just in Greece, but also in Mesopotamia, India, and China, each of them civilizations with quite a few ideas about how investigations should be pursued, and about their values and uses. This will not, then, be anything like a conventional history of ancient science, let alone one that attempts to map out the path that ancient science *had* to follow to get going: for that, I shall argue, is something of a mirage.

That indeed will be one of my main points. There are nowadays a number of quite sophisticated theories in the field to account for the evolution of human reasoning, where Tooby and Cosmides especially have done pioneering work on the modes of thinking we needed to survive during the long centuries of our life as hunter-gatherers in the Pleistocene (Tooby and Cosmides 1989, 1990, 1992; Cosmides and Tooby 1992, 1994; cf. Cooper 2001). Some have argued that the key to understanding that development is the advantages humans thereby gained in the handling of complex interpersonal relations. Reasoning, on that view, is essentially a social activity, crucial in the early stages of the evolution of *homo sapiens*, though, of course, all must acknowledge that humans have also always reasoned on their own (e.g. Humphrey 1976, 1992, 2011; Byrne and Whiten 1988; Dunbar 1996; Tomasello 1999). Some emphasize the contrast between the ideals that logicians recommend and the 'fast and frugal' procedures humans standardly use in practical situations (Gigerenzer and Goldstein 1996; Kahneman 2011). One example of a recurrent flaw would be the so-called confirmation bias, that we are far better at finding support for what we antecedently assume than at searching for and evaluating counter evidence (Kahneman, Slovic and Tversky 1982; Evans 1989; Nickerson 1998; Mercier and Sperber 2011).

But even if we may readily agree that all humans are capable of reasoning and all practise both inference and persuasion, that does not mean that the manifestations of those skills are identical across all human populations and individuals. That is a question to be investigated, whether experimentally on contemporary populations, or

historically. Not being an experimentalist myself, I construe my main task in this book to assess what we can learn about this from the historical data from ancient societies. It *might* have been the case that early investigations into the physical world all took roughly the same form once they got going, the product just of a universal human curiosity. The evidence I shall collect suggests otherwise. We shall see.

So instead of a grand general theory, these studies will offer, rather, in the first instance, a set of reflections on *their* ancient reflections on their investigations, their divergent ambitions for them, their varying responses to the interferences and constraints they faced, from political authorities, from ideologies, from their colleagues—rivals with their own agenda—and from the actual subjects they chose to investigate.

That, I hope, provides at least a thin description of my strategy in this book. But let me now outline my tactics. My first chapter will focus on a particular set of problems to do with claims to know how to deliver the truth, indeed certainty. Experts of different types figure more or less prominently in most human societies, however egalitarian they may be in what we may broadly call their political arrangements. But how individuals get to claim to be, or are treated and respected as being, experts varies, as does the range of subject matter over which they exercise their expertise, some very much in the public domain, some—as in my *Upaniṣad* example—esoteric, not to say mystificatory. In some societies the different modes of justification are discussed explicitly. My first study will be devoted to the problem of whether or how far one such mode may be related indirectly to the political institutions of the society in which it was developed. The ideal for inquiry in this first study is of one that could deliver incontrovertible proof.

Chapter 2 will tackle the modalities of ancient debates, where the idiots in the title, contrasted with experts, should be taken in their ancient Greek sense, of private persons. I shall attempt a taxonomy of debates, in terms of subject matter, degree of formality, the question of who adjudicates and what the object of the exercise may be—all with the aim of assessing the positive as well as the negative effects of controversy in the advancement of inquiry. Among the ideals in question here are those of transparency and accountability.

I turn next to heuristics, to the development of techniques of research such as anatomical dissection, to issues to do with the

development of instruments and with an awareness of a need to check the accuracy of those that were available, to the collection and use of databases, to the recognition of the possibility of idealization to tackle otherwise recalcitrant problems, and to the controversial topic of ancient experimentation. Granted that observations presuppose existing theories, in what circumstances can observations modify those theories? That is one of the hazards of heuristics as I call them, a second being that to get their discoveries accepted, the discoverers must carry at least some of their constituency with them.

My fourth study will take up the last two of the questions I mentioned at the outset, namely the ontological presuppositions of ancient investigations of various types and the values and motivations that underpin them. In connection with ancient studies we should reformulate our questions about what science studies and what it is good for. We should ask, rather, what the investigators themselves thought about what they were studying and how *they* saw its value.

A final chapter will tackle the issue that lies in the background throughout the book, namely the question of whether or not we should suppose that there are radical differences in the faculty of reason across different human populations, though it is understood that, as with linguistic skills, performance will not always match competence. This has often discussed under the rubric of the 'Great Divide', a term that has been applied principally to the breakthroughs brought about by the scientific and industrial revolutions in the West. We can hope to bring to bear our historical studies of ancient civilizations to clarify what is at stake, surveying ambitiously the history of human reasoning insofar as our evidence allows us to discuss it across time and space and pinpointing in particular the effects of literacy.

The initial stimulus for this investigation came from my invitation to give the Tarner lectures at Trinity College Cambridge in the Michaelmas Term 2012, and my first two debts of gratitude go to the Master and Fellows for that invitation and to my audiences who participated in the discussion at the final seminar. But those are just the most easily identified of my debts: what I owe to colleagues in a wide variety of fields, from ancient Greek philosophy and science, to Sinology, to social anthropology, to cognitive science, to evolutionary psychology, is far harder to pin down but absolutely fundamental. Throughout my career, based principally at Cambridge, I have been

able to draw on advice from across the field of the humanities, and I would certainly not have become interested in the problems I here focus on but for the marvellous cross-disciplinary opportunities provided by the collegiate structure of this university and of others where I have tried out my ideas. Particular thanks are due to a number of specialists who have advised me on specific problems, to Chessie Rochberg and Eleanor Robson on Mesopotamian issues, to Eivind Kahrs on Indian thought, to Christopher Cullen and my colleagues at the Needham Research Institute on Chinese studies, to Nick Humphrey on evolutionary psychology, to Nick Jardine, Simon Schaffer, and Hasok Chang especially on issues in the history and philosophy of science, and to my long-time colleagues in the study of Greek thought, Myles Burnyeat, Malcolm Schofield, David Sedley, Nick Denyer, Robert Wardy, Robin Osborne, and Simon Goldhill in particular. Whatever the merits of this study, they would have been appreciably less but for what I have learnt from those who have been so generous with their time and advice. I owe a great debt of thanks to Sue Bennett at the Needham Research Institute for considerable help in the preparation of the illustrations; and finally it is once again a pleasure to thank Peter Momtchiloff and his colleagues and readers at OUP for their support and advice.

1

Democracy and Demonstration

The problem I seek to tackle in this first investigation is the relation, if any, between democracy and demonstration, where a first difficulty stems from the polyvalence of the terms for both relata. 'Democracy' is a slogan that has been applied to many different regimes, and 'demonstration' is equally a many-headed rubric. But in two ways (at least) 'demonstration' is still normally considered essential for science, that is, first, as 'exhibition' as when we talk of an anatomical 'demonstration', and, second, as 'proof'. Do we not continue to insist that science should deliver demonstration in that latter sense? Conjectures are all very well and are no doubt necessary at an early stage of investigation. But they are just guesses. They do not, but demonstrated conclusions do, provide a solid basis for further work. So my central, ambitious, if also speculative, question is: What, if anything, does the idea of demonstrating owe to political institutions?

Some of these ideas have their origins in Greek thought and whether or how far they are exceptional can be tested against the materials we have from other ancient civilizations. I shall have some things to say about both Indian and Chinese thought, about Chinese mathematics in particular. But we have long ago now learnt to be wary of our Greek legacy, even though it has so often been held up as the triumph of rationality. So I shall focus on just what the bid to demonstrate originally amounted to in Greek thought, and how far that ambition reflects or can be related to political institutions and practices, those of democracies in particular. Some of the features of the democracies I am interested in exist also in oligarchies and to that extent the problem is the relation between demonstration and the city-state.[1] But democracies illustrate those features particularly prominently and I shall concentrate on them.

[1] The question of what Greek styles of open debate owed to practices that antedated the establishment of democratic institutions is controversial. Against Lloyd 1979 ch. 4 contrast

'Democracies' vary, to be sure, enormously. Greek democracies were both in some respects far wider, and in others more restricted, than what we call democracy in the modern world, the key point being that Greece's were participatory, not representative. There are, to be sure, a few examples of modern participatory democracies, as in the political institutions of Swiss cantons (Figure 1.1). But they are rarities. Where ancient Greece was concerned, its democracies were indeed massively restricted in that only male adult citizens had a political voice. Women, foreigners, and slaves between them made up the bulk of the population in any Greek city-state, but they were all excluded from the political process. But as for Greek democracies being far wider, the degree of participation and involvement of those who did have the rights of citizenship was staggering. It was not just a question of voting once every four years for someone to represent you, as in the system we are used to: where in fact not all MPs receive

Figure 1.1 Democracy in action in the Swiss canton of Glarus, May 2006

especially Robin Osborne, 1997 and 2010. In this chapter, I concentrate not on free speech as such, but on the development of the ideal of proof, where a further complicating factor, which I shall be mentioning in due course, relates to the relative importance of mathematical practices on the one hand, and the arguments and theories of the philosophers on the other.

a majority of the votes cast by their constituents, which in turn may be an unimpressive percentage of those eligible to vote. Our version of democracy would simply not rate by ancient Greek standards.

In Greek democracies the entire citizen body had access to the Assembly, which in Athens met on the Pnyx (Figure 1.2). Although the powers of the Athenian Assembly changed over time, as Hansen especially has shown (1983, 1991), in principle it was plenipotentiary. Athens was untypical in one respect in particular, namely that it controlled an empire, which it created out of the Delian league originally formed in 478 BCE to combat the threat of renewed Persian aggression. But it can certainly be used to shed light on what was possible in a democracy. At Athens in the 5th century BCE the Assembly's business was prepared by a Council (*Boulē*). Each of the ten tribes provided fifty members chosen by lot, and each group of fifty presided over the Council for a tenth of the year. These 'Prytaneis', as they were called, had executive responsibility and effectively ran the country on a day-to-day basis. But all the major decisions were taken by the Assembly, which in the 5th century BCE at least could, if it wished, overturn the constitution itself. In principle anyone

Figure 1.2 The Pnyx, where the Athenian Assembly met

could speak, though of course some did so more often and more influentially than others (as many scholars have emphasized, notably Finley 1983; Humphreys 1985; Ober 1989; and most recently Osborne 2010). More importantly still, the citizen body was deeply involved in implementing the decisions they took. If they voted to go to war, they had to go and fight it themselves. That concentrates the mind.

In the legal domain, too, ordinary citizens were just as deeply involved. In Athens they served in the courts as 'dicasts' who combined the roles of judge and jury. The dicasts were chosen by lot from a panel of those eligible by an elaborate procedure described in the *Constitution of the Athenians* (63–6) attributed to Aristotle, though whether he was the author is disputed. There are extant examples of the devices, known as *klērōtēria*, designed to ensure that dicasts were assigned to particular courts on a random basis (Figure 1.3). The dicasts could number in their hundreds: Socrates, for instance, was tried by a body of 501 dicasts. They had no training in the law (unlike in most other societies that have or have had advanced legal institutions, where you did not get to be a magistrate

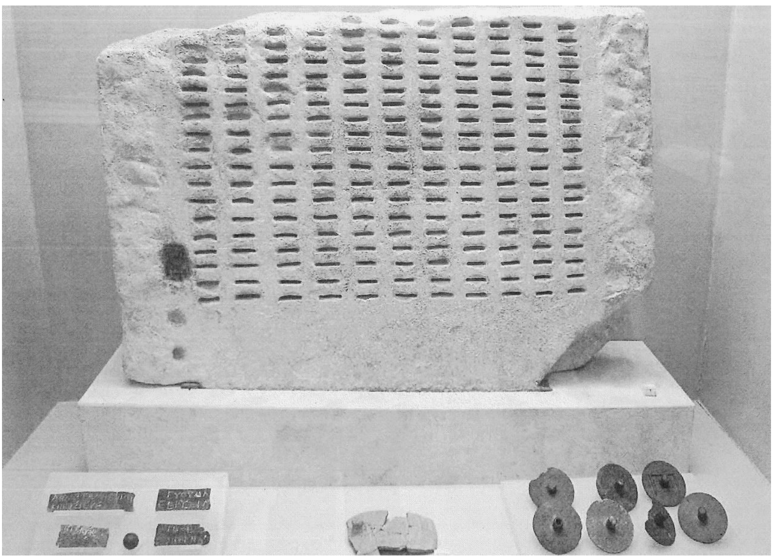


Figure 1.3 A *klērōtērion* for allotting dicasts randomly to particular courts

without some training or qualifications):[2] yet they had to decide both points of law and the guilt or innocence of the defendant. That issue was determined not by a show of hands, but by secret ballot, and again the *Constitution of the Athenians* goes into details about how this was achieved. If found guilty, the defendants could propose their own sentence as an alternative to that demanded by the prosecution: and again the dicasts decided between the two proposals by vote. Thus when Socrates was condemned, he first suggested that what he deserved was that he should be given free meals in the Prytaneum (Plato, *Apology* 36d), but then modified that to a token fine. It was only after his friends stood surety for him that he proposed a more realistic sum. Even then, one of our sources reports that the majority against him was greater for the sentence than it had been for the verdict (Diogenes Laertius 2.42). Defendants and prosecutors spoke in person (as did Socrates in his case) though by the mid 5th century they could hire professional speech-writers who would produce speeches for them to deliver. But they still delivered them themselves.

In the 4th century, membership of the Council was limited to two terms of office each of one year, so that a very large percentage of the citizen body had the experience of serving on it. Then, as we have noted, the panels of dicasts could also involve considerable numbers. Finally the Athenians acquired a probably well-deserved reputation for litigiousness, and some individuals carried on multiple civil suits simultaneously. All those factors combined to create a situation in which the actual involvement of pretty well every citizen in political and legal affairs was indeed, as I said, staggering. Even someone as reluctant to participate in politics as Socrates was represented as having found himself on the Council (chosen by lot, as I remarked). He was even chairman of the Prytaneis on the infamous occasion when, after the Athenian generals failed to pick up the dead from their crippled ships at the battle of Arginusae, there was a proposal to try them all en bloc, instead of one by one. Socrates protested that that was against the law (as it was) but he was overruled.[3] The thought that

[2] Lloyd 2009 ch. 6 gives a brief outline of the variety of legal institutions and practices for which we have evidence from both literate and non-literate societies.

[3] The evidence comes from Xenophon, *Hellenica* 1.7.15, *Memorabilia* 1.1.18, and 4.4.2; and cf. Plato, *Apology* 32b.

someone like Socrates could be the chief executive officer of the government, even just for a day, is startling. Athenian citizens may have had no formal training in ruling and judging: but they picked up plenty of experience.

But the debates in the Assembly and the law courts were seen as wildly irresponsible by Plato and to a lesser extent also by Aristotle. I may note in parenthesis that that is just about the exact opposite of what has been claimed for the *benefits* of group deliberation, the argument recently made by Mercier and Sperber (2011), for instance, that we are better at reasoning when we do it in groups. That was certainly not Plato's view, not at least where political assemblies were concerned, though maybe he would have agreed with regard to philosophy.[4] However, as we can see from the extensive extant remains of Attic oratory, the orators themselves repeatedly claim to have proved their cases. They used the very same vocabulary as Plato and Aristotle were to continue to use for their very different ideas about demonstration. The key terms were *apodeixis* and *epideixis*, which they used to suggest that they had shown 'beyond reasonable doubt', as we say, the facts of the case, or the motives of the agents, or their guilt or innocence, often adding extensive references to the 'evidence' and 'witnesses' that bore them out.[5] Indeed, that vocabulary of 'proving' can also be found in many other areas of Greek thought. Take, for example, the doctors, who deserve to be given an airing at this point.

The medical treatise *On the Nature of Man* (ch.1, *CMG* I 1.3, 164.3–166.7) starts by saying that

> he who is used to hearing speakers talk about human nature beyond its relevance to medicine will not find the present account suitable to listen to. For I do not say that a human being is all air, or fire, or water, or earth, or anything else that is not a manifest constituent in a human . . . However, those

[4] Aristotle does, however, have a convoluted discussion of the circumstances in which a collaborative judgement is superior to one taken by individuals acting on their own. One of his examples is that of judging music competitions, where different individuals can assess different aspects of the performance (*Politics* 1281a39–b10). But to express his reservations about whether a similar principle applies to all collectivities, he uses the argument that on some matters the technical expert is a better judge than ordinary lay folk (1281b38ff.), while noting the Platonic idea that the user of the end product (say a house) may be a better judge of that product than the craftsman who made it despite the superior expertise of the latter in the work of making it (1282a14ff.).

[5] For a selection of texts using that vocabulary, see Lloyd 1979: 102 n. 242.

> who make such assertions do not seem to me to have correct knowledge . . . For one of them says that this thing, the one and the all is air, but another says fire, another water, another earth, and each adds to his own speech 'evidences' and 'proofs' that amount to nothing. For when they all have the same opinion, but do not all say the same thing, it is clear that they do not know what they are talking about. One can discover this most easily by being present at their debates. When the same men debate with each other in front of the same audience, the same speaker never wins three times in succession, but now one does, now another, now whoever happens to have the glibbest tongue in front of the crowd.

That is certainly a pretty scathing attack. But what about the writer himself? In the next chapter (2, 170.3–7) he has this to say: 'I for my part will demonstrate that what I say to be the constituents of a human are, both according to convention and according to nature, always the same . . . I shall also bring proofs and declare the necessities through which each constituent increases or decreases in the body.' These constituents turn out to be the four humours, blood, phlegm, and the two kinds of bile, yellow and black. When it comes to his own demonstration of that theory, in chapter 5, 174.11ff., he points first to the differences between the humours, in colour and in temperature and humidity. And he also says that each of their powers and natures can be seen from the effects of the drugs that doctors use to expel them from the body. 'If you give a person a drug that withdraws phlegm, he will vomit you phlegm', similarly bile and black bile are purged if you give drugs that withdraw those humours and blood flows when the body is wounded 'and that happens day and night, in winter and in summer' (176.11–178.3). But while he enthusiastically exhibits the differences between the humours, he takes that to establish that those are the elementary constituents in the body. The criticism he offers of rival monistic theories could quite easily be turned against his own pluralist view: he simply assumes that the substances he identifies are elemental.

The points to take away from this text at this juncture are these: the writer is well aware that his opponents claim to have proved their theories about the physical constitution of the human body. But he dismisses those proofs as worthless. He himself by contrast has an alternative view, and again he deploys the vocabulary of demonstration to back it up. Yet in practice his arguments may seem to us as weak as those of his opponents.

In the first chapter of that treatise it was the audience who decided by acclaim who had won the debate between rival monistic theories. That is as if the merit of some theory in physics or cosmology was decided not by a peer group of physicists or cosmologists but by the public voting on the question. In Greek law and politics it was the dicasts or the citizens in the Assembly who decided by their votes which side had won the argument. But a momentous step was taken when some of the philosophers not only criticized everyone else's idea of a convincing argument but offered their alternative view of the criteria that a proper demonstration should meet. We have rigorous deductive arguments in philosophy (from Parmenides onwards) and in mathematics (for example, in the extant remains of the quadratures of lunes undertaken by Hippocrates of Chios).[6] It is one thing to produce such arguments, but quite another to analyse them and evaluate their claims to be demonstrative: that is where the 4th-century philosophers represent a breakthrough.

In Plato's view the debates in the assemblies and law courts were often not just wildly irresponsible, as I said, but intellectually flawed. The speakers may have claimed to prove their points: but all they were good for was persuasion. And the average audience could be persuaded of more or less anything—at least that was Plato's jaundiced opinion—that is, of policies whether or not they were really in the best interests of the state, of legal issues whether general or particular, whether or not their decisions were truly just (Socrates's condemnation, of course, left a deep mark on Plato). Orators are just out to gratify their audiences, he has Socrates say in the *Gorgias* 502e–503a.

> Do the orators seem to you to speak always with a view to what is best, aiming for this, namely how the citizens should be made as good as possible through their speeches: or are they too [like the poets] set on gratifying the citizens, and setting aside the

[6] The contributions of Parmenides and the Eleatics, and of pre-Euclidean mathematics (insofar as we can reconstruct this) to the development of the practice of deductive argument are surveyed in Lloyd 1979: ch. 2, where I discuss the background to the use of reductio and of modus tollens and resist any simple hypotheses to the effect that it was either philosophy or mathematics that originated those practices (as argued by Szabó and by Knorr respectively: see Szabó 1969/1978 and Knorr 1975; however, the role of the construction of the lettered diagram in Greek geometrical proof, shown by Netz 1999, was both crucial and distinctive). One major difficulty that such suggestions face is that both what counted as 'philosophy' and even what 'mathematics' should include were ill defined before the work of Plato and Aristotle. Indeed, some disputes on their interpretation continued throughout Greek antiquity.

> common interest for the sake of their own private benefit, they address the assembled people like children, trying merely to gratify them, and have no concern whatsoever whether their audience will become better or worse in consequence?

Their aim is to persuade the crowd (*ochlos*, mob, 502c9), not to instruct them. The sophist Gorgias is represented in that dialogue as claiming that he could defeat in argument any specialist in their own special field. Gorgias's brother was a doctor. But Gorgias says that he was more successful in persuading his brother's patients to take their medicine, and to submit to the knife or cautery, than his brother himself was (456b).

> Many times I have gone with my brother and other doctors to visit one of their patients who was unwilling to drink a drug or allow the doctor to use the knife or to cauterize. While the doctor was unable to persuade, I succeeded in persuasion, by nothing other than the rhetorical art.

He then claims that he could outdo any medical practitioner or indeed any other craftsman, if it came to who could persuade a general audience that they should be appointed to a public post. In Athens and elsewhere so-called 'public doctors' were sometimes appointed to be in residence in the city for a period of a year or more (though whether they were then obliged to give free treatment is unclear).[7] But in the classical period those public doctors were chosen by the Assembly, not by a board of other doctors.[8] So Gorgias continues (456b–e): 'And I further say that if an orator and a doctor were to come to any city you like, and had to compete in speech before the Assembly or some other meeting as to which of the two should be appointed as doctor, you would find the doctor was nowhere, whereas the one who is skilled in speaking would be appointed if he so wished. And if he had to compete with any other craftsman whatsoever, the orator would persuade the audience to appoint him before anyone else. For there is no subject on which the orator could not speak more persuasively than any other craftsman, before a multitude. So powerful, so strange, is the power of this art.'

That, to be sure, is a pretty extreme claim. Elsewhere Plato has another famous sophist draw a distinction that provides the basic underpinning for the justification of democracy. This is Protagoras

7 See Cohn-Haft 1956; Pleket 1995; and most recently Nutton 2004: 87.

8 In the Hellenistic period, however, *archiatroi* were chosen by other doctors; see Nutton 1988: ch. 1.

in the so-called Great Speech in the dialogue named after him (*Protagoras* 320c–328d). Where Gorgias had bracketed medicine and politics as subjects on which the orator is equally competent to speak, Protagoras contrasts the skills needed to determine technical issues with those in the political domain. Where medicine and the other arts are concerned, only a few people are in a position to give good advice, specialists in other words. But where political excellence is concerned, everyone is on a par. He tells a story (*muthos*) about how the various talents were distributed across different creatures, where each kind of animal has its own means of survival, but where humans were 'naked, unshod, unbedded, and unarmed' (321c5–6) until Prometheus stole fire and the technical skills that went with it. But that was not enough, for humans continued to kill one another.

> So Zeus, fearing that our race would be totally wiped out, sent Hermes to bring respect and justice to humans, so that cities should be orderly and should be drawn together by ties of friendship. Then Hermes asked Zeus in what way he should give justice and respect to humans: 'Am I to distribute them as I distributed the crafts? . . . For one human who possesses the medical art is sufficient for many lay people, and so too with the other craftsmen. Am I to give justice and respect to humans in a similar fashion, or distribute them to all?' 'To all,' Zeus said: 'Let all have a share, for cities could not be formed, if only a few people had a share in them as they do in the other crafts.'(*Protagoras* 322cd)

So while few are good at each specific technical skill, medicine, flute playing and so on everyone has a share in the talents you need in the political domain. So Protagoras says to Socrates, 'Your fellow-citizens reasonably accept a smith's or a cobbler's advice in public affairs' (324c).

Plato gives Protagoras some powerful arguments for egalitarianism. Yet Plato will have none of it. His view of the matter is that just as in those other technical domains you need knowledge to qualify as a competent adviser, exactly so competence in the sphere of morality and politics depends crucially on understanding. Virtue or excellence, Socrates is made to say, is knowledge or understanding. The trouble about democratic regimes is that decisions are taken by ignorant people. Worse still, they are misled by unscrupulous speakers who are themselves ignorant, concerned only with their own reputations and in winning victories in argument. What they purvey is not the truth, but what they think will please their audiences. Over and over

again we are told that their arguments produce mere persuasion (*peithō*), not (proper) demonstrations.

Yet while that contrast recurs in all sorts of contexts in Plato, and provides him with the main thrust of his argument against democracy, he did not actually define what counted for him as such a (proper) demonstration. There is even some doubt as to how far he would have seen mathematics as an adequate model. In the *Republic* (510cd)[9] at least mathematics is rated as inferior to philosophy (what he calls dialectic) on two grounds. First, it takes its hypotheses or principles (the odd and even, the figures and the three kinds of angles, for instance) for granted, and does not attempt to give an account of them. Second, it uses visible images (though in practice, as Reviel Netz (1999) decisively showed, the construction of the lettered diagram is the crucial element in Greek geometrical proof). Dialectic, Socrates is made to claim, needs no such images and it is represented as (somehow) ascending to a starting point that is no mere hypothesis, indeed an 'unhypothesized' principle, the Form of the Good.

So for the first explicit definition of strict demonstration we have to turn to Aristotle, who certainly did draw on but went beyond existing mathematical practices. Though he disagreed fundamentally with his teacher Plato on so many points (as I shall be remarking in a minute), he was on the same side (1) in being suspicious of democracy and (2) in driving a wedge between mere persuasion and proper proof. For the latter, two requirements had to be met, indemonstrable primary premises and valid deductive arguments. Your primary premises had to be indemonstrable, but self-evident: for if the premises were capable of being demonstrated, then they should be, and they would not then be *primary* premises. The indemonstrability requirement was to avoid the twin flaws of circular argument and an infinite regress.[10] As for valid deductive arguments, Aristotle analysed those in terms of his pet theory of the syllogism, though it is obvious (we should say) that

[9] At 510d1 Socrates says that geometers take their starting points 'as clear to everyone'. But whether the point is that they are indeed clear and unquestioned (as Vlastos argued, 1973), or rather that this is the geometers' *claim* rather than their *warrant* (as I suggested, 1991: 340), is disputed. Again, whether this is intended as a criticism of geometry, or merely a recognition of the methods that it has to employ, is also a matter of controversy (Robinson 1953 already took the former view, which is challenged by Burnyeat 2000: 37f). The second criticism, that geometers use visible images, i.e. diagrams, is stated at 510d5 and repeated at 511a6.

[10] Aristotle, *Posterior Analytics* I chh 1–3.

arguments do not have to be cast in canonical syllogistic form[11] in order to be valid deductions.

With this rapid survey of certain aspects of ancient Greek philosophy and society in mind, we may now assess both the strengths and weaknesses of that notion of strict demonstration and tease out its connections with democracy. On the latter question some points may be made straight away. From one point of view such demonstrations are 'democratic'. That is in the sense that in principle you only have to understand the argument to be able to judge it. Go through a proof in Euclidean mathematics (very similar to the strict demonstrations that were Aristotle's ideal, even though Euclid certainly did not cast his arguments in syllogistic form)[12] and you are led inexorably to accept the conclusion. What is to be demonstrated is announced at the outset: when the proof has been completed (via the construction of the diagram as I said), it ends off with the standard formula, Quod erat demonstrandum, *hoper edei deixai*. The truth of the conclusion in no way depends on the authority of the person who is engaged in doing the proving. That is the key point.

But if in that respect democratic, because transparent, in two other respects this notion of demonstration is anti-democratic. First as a matter of contingent historical fact, the explicit notion was hammered out (as we said by Plato and Aristotle) in order to downgrade the arguments normally used in the assemblies and law courts as *merely* persuasive. The orators may have claimed to demonstrate: but that was just legerdemain.[13]

Second, we should unmask the way in which strict demonstrations themselves are not the user-friendly tool that they might purport to be. For strict demonstrations, in the Aristotelian view, you need self-evident primary premises. But they are far harder to come by than some statements of the ideal let on. Aristotle's three kinds of

[11] Aristotle identified three figures of the syllogism, each with several moods. Thus in the mood labelled Barbara in the first figure, 'B belongs to all A, C belongs to all B, therefore C belongs to all A,' or as it is more usually represented: 'all A is B, all B is C, so all A is C,' 'all humans are mortal, all Greeks are humans, so all Greeks are mortal'.

[12] That Euclid does not in fact present his proofs in syllogistic form is obvious, even though some scholars have undertaken to show they can be reformulated after that fashion (McKirahan 1992: ch. 12, 144ff.).

[13] As we shall see in a minute, Aristotle allowed looser, so-called 'rhetorical' demonstrations, though the actual arguments the orators used often did not, in practice, meet the conditions of probability that he laid down (cf. Lloyd 1996b: ch. 1).

primary premises were axioms, definitions, and hypotheses,[14] and in Euclid (*Elements* book 1) you get a similar though not identical triad, namely definitions, postulates, and 'common opinions' (which correspond broadly to Aristotle's axioms), though how far back any such notion of axioms goes is unclear and controversial.[15] Now over a certain narrow range, there is not much to quarrel with in the claim to self-evidence. The equality axiom, which appears in Aristotle and reappears as Euclid's third common opinion, states: 'take equals from equals and equals remain'. You cannot prove that without presupposing it—which would breach the embargo against circular argument. But it can and should be taken as given.

But beyond such uncontroversial instances the self-evident status of what are laid down as primary premises is very much open to question, and this is where the chief weakness in the Greek ideal of the strictest demonstration shows up, as some Greek writers were evidently aware. Definitions and some of the other primary premises may seem innocuous, though many of those appealed to in Greek demonstrations are contentious. But the easiest way to show my point is to consider the fate of the Euclidean parallel postulate, that non-parallel straight lines meet at a point. That already seemed to some commentators in antiquity to be an interloper among the postulates. Both Ptolemy and Proclus protested that it should not be taken as an axiom, but was rather a theorem to be proved, and both of them offered proofs, only to fall foul (we should say) of the flaw of circularity.[16] So far from establishing the postulate, they presupposed it. But it was, of course, by exploring the consequences of denying the parallel postulate that non-Euclidean geometries were eventually discovered.

But if mathematics indeed provides the best sphere in which to deliver strict demonstrations in the Euclidean manner (though in fact there was plenty of Greek mathematics that did not pursue that

[14] Aristotle, *Posterior Analytics* I ch. 2.

[15] Euclid, *Elements* I. In Lloyd 1979: 113ff., I discuss the issue of the origin of the explicit notion of an axiom, and suggest that it was not available to Plato. At least, judging by what he has to say about what he called 'hypotheses' in the *Republic* 510cd, in particular the demand I have just mentioned for an 'unhypothesized' starting point for dialectic (the Form of the Good), he was not there thinking in terms of axioms, for he would presumably not have committed himself to a paradoxical unaxiomatic axiom.

[16] Both these attempts are reported in Proclus's *Commentary on the First Book of Euclid's Elements* 191.21ff, 365.5ff.

ideal),[17] we have to bear in mind that that model was adopted in many other fields as well, in anatomy and physiology (as we might call them) in Galen, and in theology in Proclus.[18] The idea was that certain conclusions could be demonstrated *more geometrico*, in the geometrical manner. But the trouble was that what were offered by way of self-evident primary premises either were anything but self-evident or they were merely vacuous. Thus Galen's indemonstrables include some highly controversial propositions,[19] three of which stand out. The idea that 'nothing occurs without a cause' had been contradicted by the Epicureans when they postulated the swerve—which is precisely an uncaused event—in order (so they hoped) to underpin free will. Again the often repeated dictum that 'nature does nothing in vain' had been denied by the earlier atomists, Leucippus and Democritus, as well as Epicurus and others, for whom it was neither necessary nor possible to appeal to final causes to explain individual phenomena or the world as a whole. A third Galenic axiomatic principle in medicine was that 'opposites are cures for opposites'. You could have that come out true, if you specified as opposites what indeed produced a cure. Yet that, of course, was circular.

But the chief anti-democratic trait in strict demonstration has yet to be flushed out. What Plato and Aristotle both sought was not just a mode of argument that would deliver truth, but one that would yield incontrovertibility. Strict demonstration was to be a knock-down argument, vulnerable to no counterargument. In Plato's political ideal, when philosophers were kings or kings philosophers, they would be in a position to prescribe the correct policies. Insofar as they were equipped with incontrovertible arguments, there would be no room for dissent, no room for further discussion even, though

[17] The tendency to privilege Euclid's *Elements* as characteristic of Greek mathematics as a whole has rightly been criticized in recent years, by such scholars as Netz 1999 and 2009; Cuomo 2001; and Tybjerg 2004. And cf. Lloyd 2012b; Mueller 2012; Netz 2012.

[18] An anonymous reader reminds me of Walzer's observation that there were Christians in late antiquity who sought to justify their faith borrowing the methods of Euclid, though some of them ran into opposition on this account from their co-religionists (Walzer 1949: 56 and 76f., referring to Eusebius *Ecclesiastical History* V 28. 13–14). Much later, when Christianity had assimilated pagan Greek philosophy and mathematics, the Jesuits certainly endeavoured to impress the Chinese on the grounds that they knew how to attain certainty, including in theological matters, while the Chinese remained ignorant of any notion of strict proof: see Gernet 1985.

[19] For debate on Galen's notion of demonstration, see Barnes 1991; Hankinson 1991; and Lloyd 2006: ch. 4.

he never *says* that. Given the structure of strict demonstration, the individual personality of the person claiming to do the demonstrating counted for nothing. The author was airbrushed out (which one might compare to the way in which authors tend to get airbrushed out of modern scientific papers). The point that Plato gave to Protagoras, that in the political domain everyone has a say (every male adult citizen, that is) that point was flatly denied in the name of setting up an elite of experts, philosopher-kings, who had unique and unchallengeable access to what is right and just and true, in Plato's case to the transcendent Forms.

Now Aristotle categorically rejected such transcendent Forms and he was highly critical of the Form of the Good, the supreme Form in Plato's metaphysics, in particular. It is neither practical nor attainable. How was someone expected to become a better craftsman or a better doctor by contemplating the Form of the Good?[20] That is Aristotle in uncharacteristic Dr Johnson mode. In any type of inquiry, he insists, we must seek for the degree of exactness that is appropriate for the subject matter. So he allows lesser, 'looser', demonstrations, including what he calls 'rhetorical demonstrations' based on probable premises, though these were still superior (in his view) to the actual demonstrations given by real-life orators, in particular because he now provides an analysis of their status and their conditions of acceptability.[21] Yet Aristotle is still wedded to the *ideal* of strict demonstration, securing incontrovertibility. That provided the gold standard, as it were, for argumentation. Strict demonstration is still contrasted with mere persuasion, even though Aristotle recognizes, in demonstration, the most persuasive mode of persuasion of all.

My argument thus far has been first that Greek-style participatory democracy was an extraordinarily favourable environment in which to study argument—I mean for the ancient Greeks to do so—and for gaining experience in it (as a Greek citizen, your skills in argument were seriously tested in everyday life far more often than we are used to); second that the techniques of persuasion that were practised in open debate provided an important stimulus for an altogether

[20] Aristotle's sharpest attack on the ethical implications of Plato's theory of forms comes in the first book of the *Nicomachean Ethics,* especially 1096a11–1097a14.

[21] I set out the evidence for a plurality of concepts and practices of demonstration in Aristotle in Lloyd 1996b: ch. 1.

different style of demonstration, since they offered negative models against which the concept of a mode of proof securing incontrovertibility could be developed; but third, that that alternative style of strict demonstration provided an ideal that was partly quite illusory, since it could not in fact be achieved in many areas of inquiry to which the ancient Greeks tried to apply it.

But is that all just idle speculation on my part? How far can we bring such conjectures about those direct and indirect links between democracy and demonstration to any kind of test? The first question is clearly to ask whether or how far other societies have developed a model of rigorous demonstration of the kind that Aristotle defined and that Euclid and Archimedes exemplified. China proves the richest source of evidence that can be brought to bear on the problem and I shall concentrate on what we can learn from that comparison. But first let me make some brief comments on another ancient civilization where we can examine the possible connections, or the lack of them, between politics and styles of proving, namely India.

India provides extensive evidence of traditions of debate and controversy, about which I shall have a good deal to say in Chapter 2. As we can see already in the *Upaniṣads*, learned sages and gurus take on other gurus in discussions on all sorts of abstruse problems, quite often in quite an aggressive way. These debates were generally held in the courts or palaces of kings or rulers, and sometimes the kings themselves join in and adjudicate, though more often it was the gurus themselves who decided who had won. The victor was the guru who reduced everyone else to silence. The debaters certainly exhibit keenness in probing alternative positions and a corresponding awareness of the need to justify some of them, though that was more often a matter of citing an accepted authority than of providing empirical evidence or conclusive argument.

Moreover, among the learned disciplines that were highly cultivated in India were both mathematics and grammar. In the latter, Pāṇini especially (whose date is disputed though most opt for the 4th century BCE) provided a model of classification, analysis, and argument that came to be influential in many other fields of inquiry. In geometry too many of the key texts are hard to date at all precisely. Estimates of that of the *Āpastamba Śulbasūtra*, for instance, vary from 500 to 100 BCE. The *Āryabhaṭīya* is generally dated to the end of the 5th or start of the 6th century CE, and Bhāskara's commentary on

it to around 630 CE. So what, we may ask, were the connections between styles of mathematical reasoning and other modes of inquiry and discussion?

The key point relates to how results are established in Indian mathematics, that is how the truth of statements or the correctness of algorithms was shown. How did they get on in geometry, for example, where considerable knowledge of what is known in the West as Pythagoras' theorem and of the so-called Rule of Three, governing proportionals, was available already in the *Āpastamba Śulbasūtra*. Ancient Indian mathematicians were confident of most of their findings, though sometimes no distinction is drawn between exact results and merely approximate ones[22] and some of their results, such as the formula in the *Āryabhaṭīya* for the volume of a pyramid, suffer not just from being approximate: they are incorrect (cf. Bronkhorst 2001: 53f.). However, these were obtained by a variety of methods that include cut-and-paste techniques, and empirical methods, such as pointing to a diagram. None of these texts proceeds by way of axiomatic-deductive demonstrations. Bronkhorst (2001: 55) indeed put it that in Bhāskara's case there is little space 'for proofs and none at all for definitions and axioms, the starting-points of perfect proofs', though Keller (2012: 507) found evidence both of the checking of rules and of seeking to convince readers of their validity. A question mark still hangs over the extent to which the practices of Indian mathematicians *imply* a notion of proof, and the explicit terms that have been translated—'proof', 'verification', and 'explanation', namely *upapatti*, *pratyayakaraṇa*, and *pratipādita* respectively—are open to divergent interpretations (as Keller 2012 discusses with great care). Yet however we resolve those issues, it is agreed on all sides that there is no trace, in any of these classical texts, of any ambition explicitly to secure mathematics on axiomatic foundations.

Puzzled by this lack, Bronkhorst tentatively suggested that it may have been the influence of Pāṇini's non-axiomatic presentation of his

[22] Examples of this would be expressions of the relationship between the side and diagonal of a square and formulae for converting a square into a circle or vice versa (see Thibault 1875: 239, 251ff.; Michaels 1978: 142ff., 153f.; Pingree 1981: 5). When precisely the terms *nitya/anitya* (the root meaning of which is regular/irregular, see Müller 1930) were deployed in Indian mathematics to express the contrast between exact and merely approximate results is disputed: see Lloyd 1990: 101–4 with references to previous literature.

analysis of linguistics that could help to account for this, though to be sure Bronkhorst is well aware that other pre-modern mathematical traditions did not develop axiomatics either—where there is no question of grammar providing an alternative model inhibiting any such development.

So the situation in India is interestingly complex. There is plenty of debate, including among mathematicians, and a considerable interest in geometry. But where in Greece the arguments of the sophists, politicians, and orators led to a negative reaction on the part of Plato and Aristotle and to their forging an ideal of reasoning that would yield incontrovertible conclusions, in India there was no such demand for a certainty that could be delivered only by axiomatization. But then the debates the gurus conducted were very different from those that attracted Plato's criticisms in one significant respect: they were not decided by majority vote, even when they took place with a lay audience present (cf. later, in Chapter 2). The gurus were not attempting to win round a general public, so often described in our hostile Greek sources as a mob, *ochlos*. The model that dominated Greek political assemblies and law courts was one where everyone in the audience had an equal say in the final decision. Indian rulers, of course, regularly consulted their ministers and advisers. Indian gurus deferred to one another, on occasion at least. But the democratic institutions we have described in Greece had no parallel in ancient India, and without the stimulus of a negative model of persuasion, there was no need to elaborate an altogether different set of ideals for strict demonstration. The hypothesis that I proposed of a connection between democracy and such demonstration in Greece would have been seriously weakened if one of the two had existed without the other in India. But as it is, neither did, and so thus far the hypothesis withstands this first test.

But what about China? Here too there is, if anything, even fuller evidence of sophisticated arguments in all sorts of contexts, including especially in both politics and mathematics. It is nonsense to assert (though we still hear it said) that Chinese mathematics was purely practical in interest and that Chinese mathematicians were hopeless in geometry, the area in which the Greeks excelled and one of the main fields in which axiomatic-deductive demonstration was employed. The main early Chinese mathematical text, the so-called *Nine Chapters of Mathematical Procedures* (*Jiuzhang suanshu*), dates from

around the turn of the millennium, and the first extant commentary on it—one of many—was written by Liu Hui, in the 3rd century CE.[23] Neither canon nor commentary has so much as a trace of an axiomatic base: but both display sophisticated argumentation.

One example would be the investigation of the area of a circle and of the relationship between the circle and the circumference, that is π. In his commentary to *Nine Chapters* I.32 (103.9ff.) Liu Hui first establishes that the area of a circle is given by the formula A= ½ c times ½ d, where A is the area, c the circumference and d the diameter. But he then proceeds to evaluate the ratio of c to d, first inscribing a hexagon in the circle, then a dodecahedron, then successively larger regular polygons, on each occasion doubling the number of sides to give increasingly close approximations to the value he was after (Figure 1.4). He envisages calculating the area of a regular polygon with 192 sides (that is beyond the capacity of an ordinary pocket calculator) and a later Chinese mathematician, Zhao Youqin in the 13th century, contemplated doing the same for a 16,384-sided regular

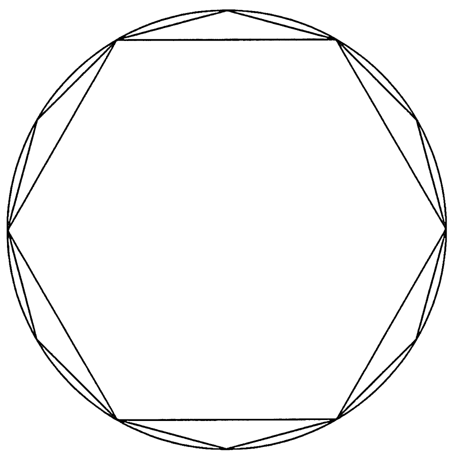


Figure 1.4 Liu Hui's procedure for approximating to the circle-circumference ratio (π), using inscribed regular polygons, starting with an inscribed regular hexagon (where the ratio is 3:1)

[23] The texts of the *Nine Chapters* and of Liu Hui's commentary are set out with extensive interpretative essays in Chemla and Guo 2004.

polygon (see Volkov 1997). This was similar, of course, to the technique Archimedes used in *On the Measurement of the Circle*, Proposition 1, when he used both inscribed and circumscribed regular polygons to approximate to the value of π, though he began with a demonstration, via reductio, that the area of the circle equals that of a right-angle triangle one of whose sides equals the radius, the other the circumference of the circle (Figure 1.5).

But the Chinese notion of *proving* in mathematics generally hinges on checking the results they obtained and more particularly on establishing that the algorithms used are correct (Chemla 2012a and 2012b). The latter move certainly happens often enough: discussing the algorithm for the addition of fractions in the *Nine Chapters* (96.1–2), Liu Hui first names the procedures involved. Every time denominators multiply a numerator that does not correspond to them, we call this 'homogenize'. Multiplying with one another the set of denominators, we call this 'equalize'. Thus to add two fractions a/b and c/d, you first cross-multiply, that is multiply a/b by d/d, and c/d by b/b. That gives you two fractions ad/bd and bc/bd which can be added together, (ad+bc)/bd, as they both share the same denominator, bd. Liu Hui then duly notes that 'the procedures cannot have lost the original quantities'. Once Chinese mathematicians were confident

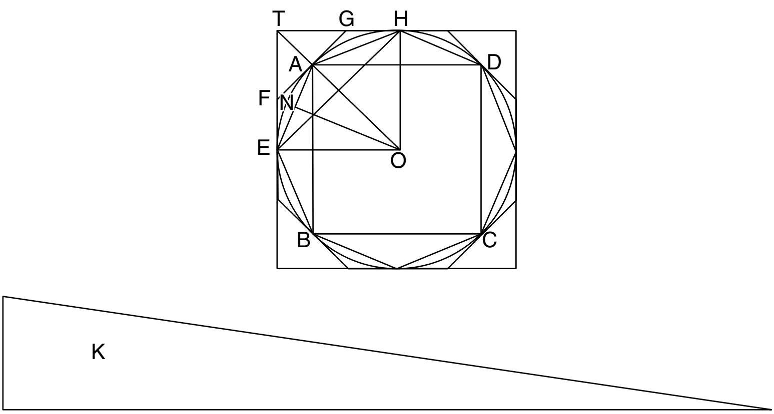


Figure 1.5 Archimedes *On the Measurement of the Circle* Proposition 1. The area of any circle is equal to that of a right-angled triangle in which one of the sides about the right angle is equal to the radius, and the other to the circumference, of the circle

their procedures were sound and yielded correct results, they did not try for incontrovertibility, but got on with the next problem.

But the Chinese data are also significant when we turn to the other context in which sustained argumentation occurs, namely in politics. Chinese philosophers often saw their chief role as being that of persuaders, but the target of their persuasion was generally the ruler, after the unification under Qin Shi Huang Di in 223 BCE, the emperor. In that context we have records of important debates, held at court, often highly formal, often in the presence of the emperor himself: I shall come back to this in Chapter 2. Of course, Chinese persuaders also went all out to defeat their philosophical rivals, though none suggested that you need incontrovertible arguments to do so, and none complains that the other side failed to produce such.

Actually much debate was conducted in writing and much was directed against past dead authorities, not living ones who could answer back, but there is plenty of evidence, starting in the Warring States period, of criticisms mounted against leading thinkers. The *Zhuangzi* compilation (dating from the 4th to 2nd centuries BCE) mocks the pomposity of Confucius's insistence on the importance of ritual. When confronted by the infamous robber Zhi (*Zhuangzi* 10 and 29), Confucius is outwitted, even when Zhi claims that robbers exemplify all the Confucian virtues. Being able to guess where the valuables are hidden, Zhi says, shows the Intuitiveness of the sage. Being first in is Courage. Being last man out is Duty. Knowing whether one can pull it off is Wisdom, and giving everyone a fair share is Goodwill (ch. 10). In the *tianxia* chapter especially (ch. 33), a whole series of authorities is criticized for their merely partial grasp of the Way. They include Lao Dan (the putative author of the *Daodejing*), Mo Di (the founder of the Mohist school), and even Zhuang Zhou (that is Zhuangzi himself). While all had some inkling of the Way, none grasped it all.[24]

Again in the 3rd century, Xunzi has a chapter entitled 'Against the Twelve', in which he attacks a list of prominent thinkers, including Mo Di, Hui Shi, Zisi (a disciple of Confucius), and even Mencius, generally considered the chief representative of Confucian thought and values after Confucius himself (Knoblock 1988–94, vol. 1, ch. 6:

[24] Precisely the same expression is used no less than five times throughout the chapter in order to make the point that while all 'got wind of' the Way and delighted in it, none grasped the whole (*Zhuangzi* 33: 16–17, 34–5, 42–3, 55, and 63–4).

212ff.). A little later in the same century the *Hanfeizi* presents two chapters (49 and 50) which criticize both Mohists and Confucians alike, and the tradition of such attacks continues into the Han, especially in the *Lun Heng* of Wang Chong.

However, no Chinese thinker ever faced a situation in which he had to convince a mass audience, where it would be that audience who would decide the outcome. Chinese debates were never settled by getting a general public to vote on the issue, nor even by public acclaim. Those democratic institutions simply did not exist in ancient China, neither in the pre-unification Warring States period, nor under the empire, even though, to repeat myself, there was plenty of argument, plenty of debate, plenty of persuasion, plenty of consultation, where emperors, rulers, ministers took advice and in which rival speakers vied to outdo each other.

When, as in the 3rd-century-BCE writer Han Fei, we have explicit Chinese reflections on argument, the focus is on how to persuade the ruler, a matter of studying his psychology, not one of examining how to convince him with incontrovertible logic.

> In general, the difficult thing about persuasion is to know the mind of the person you are persuading and to be able to fit your words to it . . . If the person you are trying to persuade is secretly out for a big profit but ostensibly claims to be interested in a virtuous name alone, and you talk to him about a reputation for virtue, he will pretend to welcome you . . . but in fact shun you. If you talk to him about making a big profit, he will secretly use your advice, but ostensibly reject you . . . The important thing in persuasion is to learn to play up the aspects that the person you are talking to is proud of and play down the aspects he is ashamed of. Thus if the person has some urgent personal desire, you should show him that it is his public duty to carry it out and urge him not to delay . . . If he is anxious to make a show of wisdom and ability, mention several proposals that are different from the one you have in mind but of the same general nature in order to supply him with ideas; then let him build on your words, but pretend that you are unaware that he is doing so, and in this way abet his wisdom. If you wish to urge a policy of peaceful coexistence, then be sure to expound it in terms of lofty ideals but also point out that it is commensurate with the ruler's personal interests.[25]

[25] *Hanfeizi* 12: 221, trans. after Burton Watson (2003).

The evidence from both Indian and Chinese thought tells us (I suggest) that *competitiveness* by itself was not enough to stimulate the development of the notion, and the practice, of strict axiomatic-deductive demonstration. But *elitism* by itself is not enough either, even though the principal theorists of such demonstration in Greece, Plato and Aristotle, were certainly members of an intellectual elite—as were the gurus of the *Upaniṣads* in ancient India and such philosophers as Mencius or Xunzi or Han Fei in ancient China. What tips the balance in the Greek situation is a particular way in which those two features are *combined*, where some members of the elite set up new modes of argument in direct opposition to other, populist, modes in which what mattered was what the majority approved. The contrast, to put it at its starkest, was to be between some way of guaranteeing the truth, on the one hand, and merely counting heads on the other. That at least is the suggestion I would propose.

However, I need to come back to the point I made earlier concerning the complex consequences of the ideal of strict demonstration. Incontrovertibility was often the Greek aim, I said, but as I also pointed out, in many fields in which it was sought it was unattainable. The axioms proposed were either vacuous or controversial: no sooner had the claim been made that the arguments were incontrovertible than it was controverted. Of course, if the Greeks had settled for axiom sets that simply met the criterion of internal consistency, that criticism could have been avoided. But the Greeks wanted far more, axiom sets that were true of the world. Looking at the way in which other civilizations developed mathematics and other fields of inquiry *without* a concern for incontrovertible foundations may help us to appreciate the negative effects of that very concern in Greek thought. Archimedes himself attests to the point. In his treatise called the *Method* he sets out one by which, he says, it will be possible 'to get a start to enable one to investigate some of the problems of mathematics by means of mechanics'.[26] This uses two assumptions, namely that a plane figure may be thought of as composed of parallel lines indefinitely close together, and second by imagining that those lines can be balanced by corresponding lines in a figure of known area. Now this procedure, Archimedes says, is 'no less useful also for the demonstration of the theorems themselves. For

[26] Archimedes, *Method*, Heiberg-Stamatis II 428.23ff.

certain things that had first become clear to me by mechanical means had afterwards to be demonstrated geometrically, since their investigation by this method did not yield a demonstration. For it is easier when we have previously acquired knowledge of the questions by the method, then to supply the demonstration, than it is to inquire into it without any previous knowledge.'

This is an extraordinary moment in the history of Western mathematics, itself a rather strange phenomenon if we take the broad sweep of the history of reason in general into account (there was not much call for axiomatic-deductive demonstration in the Pleistocene). It is because Archimedes sets such high standards for demonstration that he downgrades his method as merely heuristic. The use of indivisibles was not to be resuscitated until Cavalieri in the 17th century. To judge by Archimedes, the model of strict axiomatic-deductive demonstration that he inherited directly from Euclid, and ultimately from Aristotle, acted on the one hand as a great stimulus, insofar as it demanded proof, but on the other as a negative factor, since it inhibited heuristics: those results that could not be given strict demonstrations were not followed up. Demonstrate, the mantra was (as to some extent it still is) or be damned, or at least ignored.

Incontrovertibility is, no doubt, a fine ideal, but we have to ask ourselves where it is appropriate, where necessary, where possible,—and where we should settle for less, for arguments that are persuasive without necessarily being unassailable, for arguments that recognize the assumptions on which they are based and indeed their challengeability. Not all ancient persuasions were decided by the audience taking a vote, as in one Greek model. The issue of when experts should be the judge, and when on the contrary lay people (or idiots) should be allowed their say, the question on which Plato pronounced with such venom, that is one of the topics I shall tackle in Chapter 2. But my tentative conclusion from this first investigation is that the historical evidence suggests that while in origin the demand for strict demonstration securing incontrovertibility was inherently anti-democratic, in that it turned its back on democratic practices of persuasion, it nevertheless needed democracy as the negative stimulus to formulate the new ideal that it proposed.[27]

[27] A first sketch of the arguments in this chapter was presented at a workshop organized by Bruno Latour and attended also by Reviel Netz at the Centre Pompidou, Paris, on 21 June 2010.

2

Gurus, Experts, Idiots, and the Modalities of Debate

I began my Introduction with a passage in the *Upaniṣads* where the sage Yājñavalkya is not allowed to get away with saying that he knows the answer to a question, but has to give it. That question was about the 'string on which this world and the next, as well as all beings, are strung together', which had defeated another sage, Patañcala Kāpya, who admits he is stumped: 'That, my lord, I do not know.' But Yājñavalkya says that he knows, but is then challenged by Uddālaka: 'Of course, anyone can say "I know, I know." Tell us what precisely you know.' Yājñavalkya answers 'clearly that string is wind . . . It is on the string of wind . . . that this world and the next, as well as all beings, are strung together.' That is accepted by Uddālaka. 'Quite right, Yājñavalkya,' but he follows up with another equally abstruse question about the 'inner controller', which elicits a string of answers from Yājñavalkya.

> This self of yours who is present within but is different from the earth, whom the earth does not know, whose body is the earth, and who controls the earth from within—he is the inner controller . . . This self of yours who is present within but is different from the waters, whom the waters do not know, whose body is the waters and who controls the waters from within—he is the inner controller . . .

He goes on with a further nineteen answers, and the discussion ends: 'Thereupon, Uddālaka fell silent.'[1]

These are gurus who are locked in debate on the most esoteric issues. The exchanges can, as I noted before, be quite aggressive as the top sage gets cross-examined by one rival after another. One persistent female questioner, named Gārgī, poses a series of puzzles

[1] *Bṛhadāraṇyaka Upaniṣad* 3.7, trans Olivelle 1996: 41–4. Cf. Bronkhorst 2002.

about what the world is woven on. 'Yājñavalkya,' she said, 'tell me—since this whole world is woven back and forth on water, on what, then, is water woven back and forth?'

YĀJÑAVALKYA: 'On air, Gārgī.'
GĀRGĪ: 'On what, then, is air woven back and forth?'
YĀJÑAVALKYA: 'On the worlds of the intermediate region, Gārgī.'
GĀRGĪ: 'On what, then, are the worlds of the intermediate region woven back and forth?'

This goes on for quite a time, but then she is warned: 'Don't ask too many questions, Gārgī, or your head will shatter.' Thereupon Gārgī fell silent.[2]

In these contests, indeed, the defeat of one of the debaters is regularly said to lead to his or her head shattering or falling off. Victory comes when either the questioner runs out of questions or the answerer cannot reply. The winner is the guru who has, quite literally, the last word. That a victory has been won is beyond doubt. But *why* some answers are accepted, others not, and what in either case the grounds are, are far from clear. Nor, it seems, are apparently contradictory statements disallowed. For example, when questioned about the number of gods there are, Yājñavalkya gives seven incompatible answers, each followed up by a different justification, though in the case of the answer 'one and a half', that depends on a pun on the Sanskrit term *adhyardha*, meaning both 'one and a half' and what increases.[3]

This prompts the set of issues that I shall address in this chapter. On the view I have mentioned before, that emphasizes the key social functions that human reasoning was evolved to serve—we needed it

[2] *Bṛhadāraṇyaka Upaniṣad* 3.6, Olivelle 1996: 40–1.

[3] *Bṛhadāraṇyaka Upaniṣad* 3.9, Olivelle 1996: 46–7. Then Vidagdha Śākalya began to question him. 'Tell me, Yājñavalkya, how many gods are there?' He answered in accordance with this very ritual invocation: 'Three and three hundred, and three and three thousand.' 'Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?' 'Thirty-three.' 'Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?' 'Six.' 'Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?' 'Three.''Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?' 'Two.' 'Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?'
'One and a half.' 'Yes, of course,' he said, 'but really, Yājñavalkya, how many gods are there?' 'One.' Yājñavalkya then specifies what he meant when he gave each of these different answers, ending up with his account of the one god: 'Breath: He is called "Brahman" and "Tyad".'

(and need it) to survive as the social animals we are—debate may be expected to be more or less universal, and indeed extensive ethnographic data confirm that—while registering, of course, that the degree of intensity of engagement in discussion and argument varies. At one end of the spectrum, however, there are non-literate societies where skill in speaking is a highly prized attainment and where the various virtues and vices of good and bad speakers are recognized explicitly in an extraordinarily rich vocabulary. This was studied by Bohannan (1957) among the Tiv, for example. Another such society is the Barotse, in South Africa, commented on extensively by Gluckman (1965, 1967, 1972). The Lozi, as the people in question are called, pride themselves on their talent in argument in general, especially in the context of the law, where they consider themselves to be far superior to white people. They have distinct terms for 'being able to classify affairs', for being 'clever and of prompt decision', for a judge 'who relates matters lengthily and correctly', and for a judge 'who has good reasoning power and is able to ask searching questions'. Conversely, among terms of disapproval, are those that refer to 'speaking on matters without coming to the point', for 'wandering away from the point when speaking', and for 'getting entangled in words'.[4]

My topic here, however, is the role of debates in ancient civilizations where they differ interestingly and importantly from the point of view of the development of inquiry. How far does it seem possible to draw up something like a taxonomy of debates and to assess their potential contribution to the growth of inquiry? One key question will be where it is thought that one should defer to experts, or where lay people (idiots in the original Greek sense) should determine the outcome. And if we need experts, what makes them the experts that they are? How transparent are the criteria for expertise and to whom are they accountable? Debates, as we shall see, can be a powerful stimulus to defining disciplines, but sometimes that may degenerate simply into wars of words, where the only skill in question is skill in rhetorical persuasions. Claims to expertise meanwhile could often sound hollow—to the ancients, not just to us.

Obviously the topics are vast: but I hope to be able to illustrate at least some recurrent themes and patterns. In the matter of expertise

[4] Gluckman 1967: 276f.

we nowadays, of course, feel pretty confident that we can detect excellence in science, though there is far less of a consensus where philosophy is concerned. Nobel prizes don't get awarded to just anyone. But what about the situation before 'science' had acquired anything like its present status, indeed before science was recognized as such? As is well known, no ancient civilization had a category that corresponds to 'scientist'. They had terms that can—very roughly—be translated as 'mathematician' and 'doctor' and even 'philosopher', though that is trickier, but what those fields covered, and who had a claim to the expertise in question, were, as we shall see, hotly contested.[5] The battle for recognition was that much more open, with both the nature of the discipline and how to excel in it up for grabs.

Let me tackle first that question of a possible *taxonomy* of the modalities of ancient debates where I can pick up and develop points from my discussion in Chapter 1. Debates may be differentiated in four main ways, by subject matter, by the degree of formality in the proceedings, by who decides the outcome (the participants themselves, some adjudicator or person in authority, or a lay audience), and most importantly by aim, the purpose of the exercise.

So far as *subject matter* goes, that may vary from the most abstruse (as I have just exemplified in the *Upaniṣads*) to what should in principle be within the reach of non-specialists. Issues in metaphysics and theology come at one end of that spectrum. Agonizing over what to do in your present predicament comes at the other, for that figures in every society, though different societies have different ways of arriving at decisions on political or legal affairs. In some (notably ancient Greece), it is not just present and future policies that may be debated openly, but also the merits of different types of political arrangements themselves, as in the arguments that reverberated in classical Greek texts on the relative merits of monarchy, oligarchy, and democracy (the rule of one, of few, or of the many). One of our earliest Greek texts debating the relative merits of different constitutions comes in Herodotus (3: 80–3), when he professes to report a discussion on this held by Darius and the Persian leaders who had just

[5] Lloyd 2009: chh. 1, 2, 4, and 8 discuss in some detail the similarities and differences in how these fields were construed in different ancient and modern societies and the corresponding problems of translation, given that there is no neutral vocabulary in which the differences can be expressed.

gained power. But we should not be misled: although the context is Persian, the contents of the debate are typically Greek, as Herodotus perhaps acknowledges when he introduces his account by saying that in this council 'words were spoken which to some Greeks seem incredible'.

Then what is more interesting from the point of view of the growth of inquiry into the physical world, there is also plenty of evidence, already in ancient societies, of a variety of debates of different types in medicine and astronomy, both fields in which there were articulate rival claimants to expertise keen to impose their views on how the subject should be done.

Thus commenting on the extensive extant correspondence between the scribes and Assyrian and Babylonian kings dating from the 8th and 7th centuries BCE, Parpola distinguished several different types of 'learned scholars' (1993: xiii). First there is a general category of 'able scholars' (*Ummâni lē'ûti*), and then at least five terms for more specialized activities. These (with his tentative translations) are: (1) *Ṭupšarru*, 'Astrologer/scribe', (2) *Bārû*, 'Haruspex/diviner', (3) *Āšipu*, 'Exorcist', (4) *Asû*, 'Physician', and (5) *Kalû*, 'Lamentation chanter'. There was certainly some overlap between these. The ideas and practices of the two types of healer, *āšipu* and *ašu*, were not as distinct as those rather positivist labels might be thought to imply. Again we hear of one scribe, Marduk-šāpik-zēri, who sets out his credentials in several fields, to stake his claim to continue to be able to watch the sky and report on portents to the king (*State Archives of Assyria* X Tablet 160, Parpola 1993: 122). First he claims to follow in his father's footsteps ('I fully master my father's profession, the discipline of lamentation [as *kalû*].'). But then he continues:

> I am competent in . . . purification of the palace. I have examined healthy and sick flesh . . . I have read the *Enūma Anu Enlil* and made astronomical observations. I have read the [anomaly series] *Šumma izbu*, the [physiognomical works] . . . and the [terrestrial omen series] *Šumma ālu*.

To get to be recognized in any of the categories of 'learned scholar', you had first to serve a long apprenticeship with a master (which might be preceded by a ritual initiation) and even more importantly, as this case shows, you had to have mastered the relevant canonical texts. But where astronomers/astrologers were concerned there was a further way to impress the kings. Mostly they were interested in what the signs in the heavens foretold for events on earth, but by the time

these Letters and Reports were written, the scribes were able to predict certain celestial phenomena, for example the appearances and disappearances of planets and lunar and, some claimed, even solar eclipses. Thus one prediction of Venus once again becoming visible after a period of invisibility, made by a scribe called Nergal-eṭir, reads (*State Archives of Assyria* VIII Tablet 246, Hunger 1992: 137): 'During this month Venus will become visible in the east in Leo.' Forecasting an eclipse of the moon was relatively straightforward, given that the periodicity in question was known,[6] but some scribes pretend to be confident in the far more difficult case of solar eclipses, where the chief complication was their varying visibility from different points on the earth's surface. Thus one wrote (*State Archives of Assyria* X Tablet 170, Parpola 1993: 130): 'Concerning the solar eclipse about which the king wrote to me: "Will it or will it not take place? Send definite word," an eclipse of the sun, like one of the moon, never escapes me.'

Obviously a lot of prestige attached to being able to forecast an eclipse and to get it right. A one-off correct prediction might be put down to chance, but repeated successes were an impressive token of expertise. Yet that was not the only object of the exercise, since the next question was what the eclipse *meant* for the king, for the state, or for its neighbours. Even when the phenomena were known to be regular, they were still considered to be signs (Rochberg 2004).

We know of plenty of Babylonian and even earlier Sumerian debates often cast in the form of fables, as for example that between the 'tamarisk' and the 'palm', which Lambert (1960: ch. 7) dubbed 'contest literature'. While we do not have Mesopotamian tablets that describe astronomical debates as such, there are plenty that certainly refer to the polemics between rival scribes as they make claim and counterclaim about what was happening in the heavens. Two tablets describe the dispute over whether in fact Venus was visible, where knowledge of its periodicities was evidently at stake. One reads (*State Archives of Assyria* X Tablet 51, Parpola 1993: 37–8)

> Concerning the planet Venus about which the king, my lord, wrote to me: 'I am told that it has become visible,' the man who wrote

[6] One scribe writes (*State Archives of Assyria* VIII Tablet 388; Hunger 1992: 222 and 224): 'On the 14th day the moon will make an eclipse. It predicts evil for Elam and the Westland, good for the king, my lord. Let the king, my lord, be happy. Already when Venus was visible, I said to the king, my lord: "An eclipse will take place." From Rašil the older, servant of the king'.

> this . . . is in complete ignorance . . . Venus is not yet visible. Tonight, as I am sending this message to the king, my lord, we see only Mercury: we do not see Venus. Presently it should be situated under Aries, in opposition to Saturn.

And a second is equally contemptuous of the mistakes of a rival (*State Archives of Assyria* X Tablet 72, Parpola 1993: 54): 'He who wrote to the king, my lord, "The planet Venus is visible" is a vile man, an ignoramus, a cheat. And he who wrote to the king, my lord, "Venus is rising in the constellation Aries" does not speak the truth either. Venus is not yet visible . . . He does not know the cycle or the synodic revolutions of Venus. Why does someone tell lies and boast about it? If he does not know he should keep his mouth shut.'

Many debates were highly *formal* affairs. In both India and China many were held in the courts or palaces of rulers. Where you sat could be important, for it reflected your status and authority. There is a good example of that in one well-documented Chinese debate on an astronomical subject, that held in 175 CE where the issue was the date of the epoch from which the calendar was calculated (Cullen 2007: 257f.). That sounds and is pretty technical, but we should not miss the political implications. If the calendar got out of step with the seasons, that reflected badly on the Emperor, who was ultimately responsible not just for good order in the state, but also for harmonious relations between heaven and earth and man. But this is how the Commentary to the *Hou Han Shu* (*zhi* 2: 3037–8) describes the set-up for the debate:

> On the third month, the ninth day, all the officials met in the lower part of the public hall . . . facing east. The Commandant faced south, and the Palace Attendants, Leader of the Court Gentlemen, Grand Masters, and those with emoluments of 1,000 and 600 piculs faced north in serried ranks. The Court Gentlemen for Consultation and the Erudits faced west. A clerk . . . was placed in the middle of the seats and read out the edict and the public *yi* [reports submitted in writing]. Cai Yong came forward and sat to the northwest of the Palace Attendants . . . Then he joined with Guang and Huang in raising problems and questions with one another on the rights and wrongs of the matter (trans. Cullen 2007: 257f.) (Figure 2.1).

In such circumstances the outcome of the debate could reflect the rank of participants as much as the arguments they used for their case (cf. Lloyd and Sivin 2002: 78). I shall have more to say about this debate later.

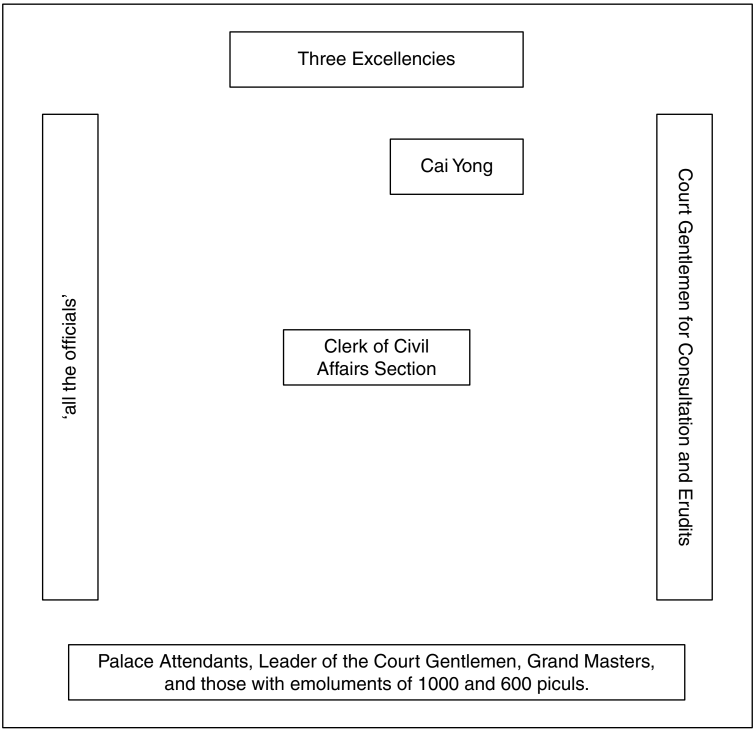


Figure 2.1 The layout of the Chinese astronomical debate of 175 CE, as described in the *Hou Han Shu zhi* 2: 3037–8

Many debates were governed by more or less explicit rules. In the exchanges between gurus in the *Upaniṣads* the questioner was not supposed to put a question to which he or she did not know the answer, and as we saw, that claim—to know the answer—could be challenged and the guru had to come up with a reply. In Greek law courts, prosecution and defence were each allowed the same amount of time, controlled by a water clock, a situation which is reflected (though not with water clocks) in many other contexts, including in the debates that are represented in just about every genre of Greek literature, in historiography, and in both tragedy and comedy especially.

In Greek dialectic, it was accepted that each person took turns in the roles of questioner and answerer:[7] you were not allowed to specify in

[7] See, for example, Plato, *Protagoras* 338de, 348a; *Gorgias* 458ab, 462a, 474b, 506c; *Republic* 350c–e.

advance the kind of answer you required;[8] ambiguous and compound questions were disallowed.[9] Nor should you bully your opponent. Mocking him or descending into verbal abuse was not to count as a refutation, at least in a dialectical exchange.[10] In hostile debates, however, all sorts of dirty tricks were allowed.

In India, too, each of the main philosophical schools, Nyāya, Buddhist, and Jain, give their own elaborate classifications of debates, distinguishing, for example, between 'good' ones directed at the truth, and 'bad' ones that aimed just at victory, and again differentiating the opponents faced and even the audiences before whom the debate was conducted. Thus opponents were classified as 'superior', 'equal', and 'inferior', and audiences as 'friendly', 'neutral', and 'hostile' (Matilal 1998: ch. 2; cf. Prets 2001 and 2003). One of the earlier and more elaborate taxonomies comes in the medical classic, the *Caraka-saṃhitā*, a compilation put together over many centuries, although it achieved the status of a canon by around the 2nd century CE. This first advises that one should not take on a superior opponent and then has this to say about how to defeat various kinds of inferior ones:

> He should overcome an unlearned [opponent] by long citations of *sūtras*, [he should overpower] an [opponent] who is weak in theoretical knowledge by [the use] of sentences containing troublesome words; an [opponent] who is unable to retain sentences, by a continuous series of sentences composed of long-strung *sūtras*; an [opponent] devoid of presence of mind, by the repetition of the same [words] with a difference of meaning; an [opponent] devoid of eloquence, by pointing to half-uttered sentences; an [opponent] devoid of self-confidence, by embarrassing [him]; an [opponent] of irritable temper, by putting him to exertion; one who is frightened, by terrifying [him]; [and] an inattentive [opponent], by reprehending him. In these ways he should overpower an inferior opponent quickly.[11]

[8] See, for example, Plato, *Republic* 337ab.

[9] See, for example, Plato, *Euthydemus* 275d, 277d ff., 295bc; *Gorgias* 466cd.

[10] See, for example, Plato, *Gorgias* 457d; *Republic* 343a ff. *Gorgias* 473de points out that neither attempts to frighten an opponent nor mocking him constitute a refutation.

[11] *Caraka-Saṃhitā* Book III ch. 8. This is the paraphrase in Prets 2000: 369–71, cited by Bronkhorst 2007: 278. Compare the versions in Sharma and Dash 1976–7, Vol. 2: 230, and Sharma 1981–3, Vol. 1: 358.

Again in Greek dialectic, though not, as we saw, in the *Upaniṣads*, inconsistency was not, in principle, allowed. Indeed, the commonest mode of Greek refutation in both oratory and philosophy was to show that your opponent had contradicted himself. Both Lysias (e.g. III 24) and Antiphon (e.g. I 10, V 36f., 49f, 54) exemplify that, while in Plato, 'contradicting oneself' is held up as not just a logical but a major psychological flaw (e.g. *Gorgias* 482bc).

Now as to *who decides*. As we saw, in the *Upaniṣads* it is the gurus themselves who judge the outcome, but they are a law unto themselves.[12] Here transparency and accountability are at a minimum. In Chinese court debates it was the ruler or his ministers who had the final say. In the discussion in 175 CE, the outcome was settled by the Emperor's ministers, and since the defeated party were judged to have proposed views that subverted good order in the state, they were condemned to receive 'heavy punishment for empty deceptions', although the Emperor stepped in and commuted the sentence. He was ultimately responsible, not just for the fate of the contending parties, but indeed in many cases for deciding who had had the better of the debate.

So Chinese persuaders had to be exceptionally wary. The ultimate target of their persuasions was the one person whose opinion overruled everyone else's. But the ruler was surrounded by ministers, and if, as often happened, you presented your case not orally, but in a written document, as a so-called memorial to the throne, that might or might not reach its intended recipient. There are plenty of cases where they were intercepted by interested parties—so in such instances the debate never got going.[13] Persuaders learnt that they had not only to study the psychology of the Emperor himself, but equally that of those by whom he was surrounded and who might get at the Emperor before you yourself could. In the 11th century the

[12] In the *Caraka-Saṃhitā* the audience, which may be more or less learned, as well as more or less friendly or hostile, has a role in setting what is to be debated (you should try to get them to decide on a topic where you are confident of your own position). But there is no suggestion that it is a lay audience who should decide the outcome.

[13] There is a notable instance of this in the biography of Li Si, who had been chief minister of Qin Shi Huang Di, in Sima Qian's *Shiji* ch. 87. When Qin Shi Huang Di died, there was a struggle between Li Si and Zhao Gao as to who should control his successor 'Second Generation'. Zhao Gao had Li Si imprisoned, and when Li Si attempted to communicate with Second Generation, in a letter quoted extensively by Sima Qian, Zhao Gao made sure that letter never reached its destination.

polymath Shen Gua presented proposals for much needed calendar reform, only to find them blocked by his enemies at court, from which indeed he was banished.[14] He was a brilliant astronomer (among other things) but he was not much good at handling those who needed persuading. To do better there, he needed to have taken a leaf or two out of the recommendations of Han Fei whose analysis of the psychology of persuasion I cited in Chapter 1. I mention this case to show that it was not just the likes of Galileo who encountered institutional obstacles in introducing innovations in astronomy, though no ancient Chinese thinker had anything like the Catholic Church to contend with.[15]

Yet another earlier episode from Chinese astronomical debate underlines yet another complicating factor. In 103 CE, Jia Kui took on the conservatives in the Astronomical Bureau on the issue of whether the longitudinal movements of the sun and moon should be measured along the ecliptic or the equator. He even introduced a new bronze armillary with an ecliptic ring to support his view that that was the more accurate method. We have a fine illustration of the way in which debate can be the vehicle for presenting innovative ideas: but although Jia Kui won the argument, his opponents in the bureau simply ignored his result and refused to use the instrument on the grounds that it was too complicated and not what they were used to.[16] A brilliant innovation was here stillborn for reasons that had nothing to do with the actual merits of the case—Jia Kui would have said.

[14] Shen Gua's achievements in a variety of fields of knowledge, and his ultimate disgrace and impeachment, are discussed in detail in Sivin 1995a: III.

[15] The ancient Greek rejection of Aristarchus's heliocentric theory, which I shall discuss in Chapter 3, differs, in that those who objected to that acted on their own behalf, not on that of state or religious institutions. This is true even when Plutarch (*On the Face of the Moon* 6: 923a) reports that the Stoic Cleanthes objected to the theory on the grounds that it was impious to suggest that the Hearth of the Universe (that is the earth) should be set in motion.

[16] Cullen 2007: 249 comments:

> Finally in 103 CE Jia Kui's campaign paid off. An edict was issued commanding the construction of a new bronze armillary sphere with an ecliptic ring (*Hou Han Shu*, *zhi* 2, 3030) . . . So had he won at last? No, not quite. The Clerks had a trick up their sleeves. They simply claimed that although the new instrument was (no doubt) more accurate than its predecessors, it was just too complicated to use because of the problem of adjusting the extra ring.

With the exception of the equatorial armillary (see below, Chapter 3), which is a single bronze ring set in the plane of the equator to display the time of the equinox, armillaries are complexes of metal rings corresponding to the great circles of the celestial sphere. Jia Kui's innovation would be to introduce a ring to represent the ecliptic.

But if many debates were decided by authorities who had the ear of rulers but were otherwise unaccountable, at the other end of the spectrum there are those Greek debates that were held before, and adjudicated by, a mass audience. That was the standard practice in Greek political assemblies, whether the decision was taken by acclaim as in oligarchic Sparta, or by secret ballot as in Athenian law courts, as I pointed out in Chapter 1. I also mentioned the complaint we find in the Hippocratic treatise *On the Nature of Man* that debates between philosophers, and again between doctors, on the constituents of the human body, were won by the speaker who had 'the glibbest tongue in front of the crowd'. The idea that a lay audience could adjudicate that issue no doubt seems bizarre to us (though we seem ready enough to take polls about who the greatest scientist was or which is the 'best' poem in the English language). But then we have to appreciate that in most areas the gap between experts and idiots was far smaller in antiquity, and just who counted as an expert was regularly contested.

There is a nice illustration of this in Aristotle's zoology. Who, one might ask, should be considered experts about bees or about fish? Beekeepers and fishermen, one might think. Not according to Aristotle. He certainly uses what he learns from consulting them, as he does extensively, but he then sometimes promptly downgrades their know-how. Fishermen are just interested in their catch:

> What contributes to their being mistaken is the speed with which such fish copulate, with the result that many fishermen never see this happening. For none of them ever watches this sort of thing for the sake of knowledge. All the same, the copulation has been observed. The fish copulate in the same way as dolphins do . . . Dolphins, however, take longer to emit the sperm, whereas fish of this sort do it quickly. The fishermen do not notice, but they do remark the gulping down of the milt and eggs by the female, so they tell the silly much repeated tale told by Herodotus the storyteller [*muthologos*] to the effect that fish conceive by gulping down the milt. It never occurs to them that this is impossible, as of course it is, because the passage whose entrance is through the mouth goes down to the stomach, not into the uterus, and whatever goes down to the stomach must necessarily be digested . . . The uterus, however, is evidently full of eggs: so how did they get there? (*On the Generation of Animals* 756a31ff.)

So fishermen who are not interested in knowledge for its own sake make all sorts of mistakes, whereas Aristotle can put them right, having observed the different lengths of time that different sea creatures spend in copulation and dismissing the idea that fish conceive by swallowing the milt on a simple anatomical argument which any fishmonger could have confirmed. On what basis would Aristotle claim superiority here? First he would insist on the importance of deliberate, sustained research, and then more particularly that the inquiry should be directed at causes, especially the final cause. Fishermen, on that score, do not rate.

Yet in his famous, or rather notorious, discussion of the reproduction of bees (*On the Generation of Animals* 3: ch. 10) we see that Aristotle himself sometimes hesitates. He first argues that 'bees' cannot be female, since nature does not give defensive weapons to female creatures, and yet bees have a sting; and then that they are not male, because male creatures do not look after their young. Yet that is a principle he knows to be subject to exceptions, as he points out in the case of the catfish now named after him, Parasilurus Aristotelis, where the male does look after its young (*History of Animals* 9: ch. 37, 621a20ff.). That was an observation that some scholars thought Aristotle had simply made up, since it does not tally with the behaviour of Silurus Glanis, the common sheat-fish found in European rivers. But then Louis Agassiz confirmed that it was indeed behaviour observed in some species that are found in North America but also in the rivers of Greece. But in this chapter of *On the Generation of Animals*, accepting that the bees are neither male nor female, Aristotle is led into a tortuous discussion of the different ways in which they, the drones and the queens (he calls them kings or leaders), reproduce: and yet he ends with this (760b27ff.):

> This then seems to be what happens with regard to the generation of bees, judging from theory and from what are thought to be the facts about them. However, the facts have not been sufficiently ascertained, and if ever they are ascertained, then we must trust perception rather than theories, and theories too, so long as what they show agrees with the phenomena.

Now we come to the key topic of *aims*, where once again there is a spectrum from entertainment, through finding out the truth, to religious conversion. We can illustrate the last with another Indian

document, a remarkable Pali text dating from the 2nd or early 1st century BCE, the so-called *Milindapañha*, or *The Questions of Milinda*. That reveals that some Indian teachers were aware of the debates of the Greeks, even though they certainly thought they were better at it themselves. Here a Greek sage, Milinda, first defeats an Indian guru but then meets a Buddhist monk named Nāgasena. Milinda is the Indian version of the Greek name Menander, ruler of the kingdom of Bactria founded by Alexander the Great in the 4th century BCE. What is striking here is that the idea of such a debate shows an awareness of Greek models, as Bronkhorst (1999) pointed out, even while the content of the discussion is very largely Indian. Many of the dilemmas debated relate to the Buddha himself or to Nirvana. In answer to a question about the root of time, past, present, and future, the reply is 'Ignorance', not something you would expect from a Greek philosopher.[17] Moreover, the outcome is very different from the usual run of Greek discussions, for once Milinda has been overcome by Nāgasena's superior arguments, he converts to Buddhism. The text we have was, indeed, a formidable weapon in Buddhist proselytism, being translated first into Chinese in the 3rd century CE, then later into Korean and Japanese, all with the aim of securing converts.

At the opposite end of the spectrum, some debates were largely for entertainment. That too is attested in ancient India, where some of the contests between rival gurus were held for the delectation of rulers. In Greece, some sophistic displays were just for show, indeed for showing off. Aristotle reports (*Rhetoric* 1419b3f.) that Gorgias recommended that you destroy your opponents' seriousness with laughter, and their laughter with your seriousness. We are told in Plato's *Republic* (539b) that the young treated the argumentative exchanges of the sophists as sport, something of which Socrates, of course, deeply disapproved. He puts it: 'I imagine you have not failed to

[17] The King said: 'What is the root, Nāgasena, of past time, and what of present, and what of future time?' To which he replies: 'Ignorance. By reason of Ignorance came the Confections, by reason of the Confections consciousness, by reason of consciousness name-and-form, by reason of name-and-form the six organs of sense, by reason of them contact, by reason of contact sensation, by reason of sensation thirst, by reason of thirst craving, by reason of craving becoming, by reason of becoming birth, by reason of birth old age and death, grief, lamentation, sorrow, pain and despair. Thus it is that the ultimate point in the past of all this time is not apparent.' To which the King observes: You are ready, Nāgasena, in reply. (*The Questions of Milinda*: trans. T. W. Rhys Davids (1890), Book 2, ch. 3: 79).

notice that lads, once they get a taste for speeches, misuse it as a form of sport, always employing it contentiously, and, imitating confuters, they themselves confute others. They take delight like puppies in pulling about and tearing with words all who come close to them.' Again, in Plato's *Euthydemus*, two sophists run rings round a young man, leading Socrates to expostulate: When will they start the serious business of persuading him that he ought to philosophize and practise virtue?[18]

Much intellectual energy was spent in India, China, and Greece on paradoxes and puzzle solving. Our Chinese sources provide evidence of practices known as 'stumping' (*he nan*) or 'quizzing' (*nan jie*). One of the more detailed accounts relates to a young civil servant named Dai Ping (1st century CE). When at a New Year's Day celebration with all the high officials in attendance, the Emperor commanded his ministers to 'quiz' each other on the classics, Dai Ping surpassed himself. Whenever someone did not know the meaning of something, his sitting mat was taken away and added to those of the one who did. Eventually Dai was sitting on a pile of more than fifty (*Hou Han Shu* 79A: 2553–4; cf. Lloyd and Sivin 2002: 69).

Yet light-hearted exchanges could sometimes have a serious underlying purpose. The Delian problem was the riddle of how to double a cube (the Delians were told they had to build an altar double the size of an existing one): but though that may look rather like the riddle of the Sphinx, it stimulated some serious mathematical explorations, just as squaring the circle did. Indeed, as Reviel Netz (2009) has shown in the Hellenistic period especially (but also before), puzzles and their solutions created a genre he dubbed 'Ludic Proof'. Archimedes, no less, developed a sophisticated arithmetical notation to express large numbers in the work called the *Sand Reckoner*, but his starting point there was the folksy problem of whether one can number the grains of sand that the universe would hold.

[18] Socrates: Then you two, Dionysodorus, I said, would be the best persons there are to encourage one to philosophy and the pursuit of virtue. Dionysodorus: We think so at least. Socrates: Well then, please defer the exhibition of other matters to some other occasion, but display this one thing. You are to persuade this young fellow here that he ought to philosophize and practise virtue and so you will oblige me and all those present. (Plato, *Euthydemus* 274e–275a)

But other aims were also often in view. From the perspective of an Indian rajah a debate might just be for amusement, but for the contestants themselves what was at stake was their reputation, their honour, even their livelihood. A defeated guru was humiliated and became the servant of the guru who had beaten him, confined indeed to eating the leftovers from that guru's table (Bronkhorst 2002). That type of humiliation is rare in Greece, though when the Sphinx was defeated by Oedipus, she took her own life (in one story) or (in another) was killed by him.[19] But success in argument was certainly a way for a budding Greek sophist to make a name for himself—and to attract fee-paying pupils.

In Plato, the sophist Hippias is said to be prepared to discourse on just about any subject that anyone raised (*Hippias Major* 285b ff.; *Protagoras* 315c, 318d ff.). Protagoras is made to claim that whether it is a matter of full-scale disquisitions or brief question and answer sessions, he can excel (Plato, *Protagoras* 329b, 334e–335a). But of course, he was no match for Socrates. He for his part is made to claim that he is not interested in victory, but solely in the truth (*Gorgias* 473b). While disputants can always be refuted, the truth can never be—so here were debates aiming to get at the truth.[20] We may note, in passing, something of a paradox. While, as we saw in Chapter 1, Plato rates mere persuasion far below demonstration, the vehicle he uses to make that point, and indeed to get at the truth on any subject, is the dialogue, interpersonal exchanges, in other words, however much they target an impersonal, objective, incontrovertible conclusion.

Plato was deeply suspicious of rhetoric, but Aristotle sets about restoring it to its rightful position as a genuine art, as the faculty of discovering the means of persuasion on any subject whatsoever, the counterpart to dialectic which he downgraded from the supreme position it had occupied in Plato as the study that yielded understanding of transcendent Forms. At the start of the *Rhetoric* (1354a1ff.),

[19] Apollodorus, *The Library*, 3. 5. 8 reports her committing suicide, while the story of Oedipus killing her is in Corinna, Fr 672, Page 1962: 340.

[20] Socrates: You at least said that wrongdoers are happy, if they pay no penalty.Polus: Certainly. Socrates: Whereas I say they are most wretched, and those who pay the penalty, less so. Do you wish to refute that also? Polus: Why that is still harder to refute . . . , Socrates. Socrates: Not merely so, Polus, but impossible. For the truth is never refuted. (*Gorgias* 473b)

Aristotle says that 'Rhetoric is a counterpart to dialectic: for both have to do with matters that everyone can recognize and are not confined to any special branch of knowledge. So everyone in a way shares in both.' And why? Because 'all men, up to a point, attempt to criticize and maintain an argument, to defend themselves and to accuse'— eloquent testimony to Aristotle's perception of the argumentativeness of his fellow Greeks. Some might even say that this anticipates those moderns, such as Mercier and Sperber (2011), who see persuasion as the basic function of reasoning, or those such as Dascal and Chang (2007), who emphasize how controversy may stimulate innovation. The collection of essays in Dascal and Boantza (2011) presents a number of cases that illustrate this, where controversies were the driving force in what we call the scientific revolution, which pitted Fabri against Galileo, and Leibniz against Newton, while the most famous case of all, perhaps, was the quarrel between Boyle and Hobbes (Shapin and Schaffer 1985). However, for Aristotle and most Greeks it would be cooperative dialectic that would serve such a role, not contentious eristic.

Indeed, the treatise Aristotle devoted to dialectic in his sense, the *Topics*, throws interesting light on his expectations. At the start of the treatise (100a27ff.), he first differentiates demonstrative from dialectical arguments. 'Reasoning is demonstrative when it proceeds from premises that are true and primary . . . Reasoning is dialectical when it proceeds from generally accepted opinions,' that is from probable or plausible ones, or from ones your partner in discussion accepts. Yet it is still a cooperative endeavour, to explore the strengths and weaknesses of different positions. That differentiates these arguments from contentious ones, based on opinions that are not generally accepted but just masquerade as such. Sophistic reasoning, he elsewhere states (*On Sophistical Refutations* 171b25ff.; cf. *Rhetoric* 1355b17ff.), was aimed just at reputation and money, since the sophists' income depended on this: while eristic debates were directed simply at scoring a victory, by fair means or indeed by foul. These are fine distinctions, similar to those we find in Indian philosophy (Matilal 1998: 2ff. and ch. 2), though to be sure they were always difficult to apply in practice.

When Aristotle turns to explain how he hopes his own discussion of dialectic will be useful (*Topics* 101a25ff.), we have three possible functions distinguished, giving us his views on my fourth question, namely the taxonomy of aims. First there is the training or gymnastic function. Then it is useful for exchanges where you need to deal with

people on the basis of their own opinions. Most interestingly, a third function is said to be for the study of the philosophical branches of knowledge:

> For the philosophical branches of knowledge it is useful because, if we are able to raise difficulties on both sides, we shall more easily discern both truth and falsehood on every matter. Moreover, it is useful with regard to what is primary in each branch . . . This process belongs peculiarly or most appropriately to dialectic, for being characterized by examination, it provides a path towards the principles of all methods of inquiry.

The first point under this heading suggests that dialectic explores both sides to any given question, but the second, maybe more fundamental, role, is that since the primary premises of demonstrations are not given by the subject itself, one way to get at them is via the collaborative investigation into possible starting points.

How does that work in practice? We can follow up the ways in which debates were, and were not, constructive and helpful by considering some examples from medicine. Debate and dialogue figure over and over again in our ancient sources, discussions between doctors and patients, debates between different doctors on particular cases or on general theories, arguments that doctors used to justify their claim to expertise and to undermine those of their rivals. The expert/idiot boundary was particularly contentious in medicine, and in Greece, as we shall see, doctors faced some radical sceptics who denied that there was such a thing as an art of medicine in the first place. That certainly put the doctors on their mettle.

But let me start with a Chinese example of medical debate from the biography of the doctor Chunyu Yi in the first great universal dynastic history, the *Shiji* of Sima Qian around 90 BCE. Chunyu Yi himself lived some sixty years earlier. In this account, Chunyu Yi is careful to distinguish himself from the mass of ordinary or common doctors (*zhong yi* or *su yi*), and one of the ways he states his credentials is by citing the canons he has studied.[21] But on one occasion he confronts a

[21] Chunyu Yi first cites his teacher Yang Qing: 'I wish to take my books of secret formulae in their entirety and teach them all to you.' Then Chunyu Yi himself goes on: 'Your servant, Yi, thereupon said: "I am very fortunate. This is more than what I, Yi, had dared to hope for." Your servant, Yi, then refused the mat [a gesture of respect] and venerated him repeatedly. I formally received his "Pulse Book", "The Upper and Lower Canon", "Diagnosis by means of the Five Colours", "The Art of the Irregular and

fellow practitioner, named Sui. When that doctor cites as his authority the legendary figure Bian Que, who was reputed to have brought people back from the dead, Chunyu Yi agrees that he has quoted Bian Que correctly, but goes on to insist that he had got the message quite wrong.

> What you are discussing, Sir, is far off the mark. Although Bian Que's word is like this, you still have to examine it with care, establish the measures of length and weight . . . calibrate weight and scale . . . combine the models of colour and pulse, of the outer and inner, of having excess or being insufficient, and of the smooth and contravective . . . Then only can you provide a proper discourse.[22]

Bian Que's position as an authority is not called into question, but the understanding of his teaching is. You have to be able to interpret your authorities correctly, a matter of judgement, based on personal experience, where you could easily be mistaken.

Many Greek examples portray a situation in which rival doctors engage in debate in front of their patients and their relatives and friends. Such Hippocratic treatises as *On Diseases I* and *Decorum* provide tips on how to deal with this. Thus *On Diseases I*, ch. 1, L. VI 140.1ff., offers advice for anyone 'who wishes to ask questions correctly and to answer the questioner and to debate correctly, on the subject of healing'. 'Whatever mistakes anyone makes either in speaking or in asking questions or answering . . . one may . . . attack him in reply in this way.' Again, *Decorum* ch. 3, *CMG* I 1 25.25ff., says that the ideal doctor should be 'severe in encounters, ready in reply, harsh towards opposition . . . silent in the face of disturbances, reasonable and resolute in the face of silences'. It seems that it is not just other doctors who are expected to raise questions: everyone, it seems, can join in and the doctor must be prepared for all eventualities.

Some treatises suggest the positive enlisting of the patients themselves who were, to be sure, the chief source of information about what they were suffering from. *On Ancient Medicine* insists that the doctor should be intelligible to those he treats. 'It seems to me especially necessary that one who discusses this art should say things familiar to ordinary people (*dēmotai*). For the subject of inquiry and discussion is simply and solely the sufferings of none other than these

Regular", "Gauging and Measuring the External Anomalies of *Yin* and *Yang*", "The Discourse on Drugs", "The Deities of the Stone" and "The Secret Book of Joining *yin* and *yang*".' (*Shiji* 102: 2796–7; trans. Hsu 2010: 73).

[22] *Shiji* 102: 2810–11; trans. Hsu 2010: 87.

same persons when they are sick or in pain.'[23] This author has a view that medicine originates in dietetics: it has a tried and tested empirical method unlike the study of 'things in the heavens and things under the earth'. Medicine has no need of what he calls *hypotheseis* (that is postulates) where it is not clear 'either to the speaker or to his audience whether what was said was true or not'. That is a swipe at astronomy, but it is also an attack on doctors who thought medicine could be based on a small number of principles such as the hot, the cold, the wet, and the dry. Again we should note that the arguments are imagined as taking place in public, with a lay audience present.[24]

But if that medical writer sets up an ideal of medicine as intelligible to anyone (and in that unlike astronomy), others are more concerned to drive a wedge between doctors and lay people. The writer of *On Regimen in Acute Diseases* ch. 1, L. II 224.2ff. criticizes some of his colleagues in a revealing way. His opponents are the authors of the now lost work *Cnidian Sentences*. They have

> correctly described the experiences of patients in individual diseases and the outcomes of some of them. Up to that point even a lay person could give a correct account, by carefully inquiring from each patient the nature of their sufferings. But much of what a doctor should know in addition, without the patient's telling him, they have omitted.

Anyone, he says, can learn the names of diseases and of standard treatments, but a correct understanding (he means via his own complex taxonomy) is beyond them, and even beyond some of his colleagues. So he scores a double point, first against ordinary lay idiots, and then against those of his colleagues who are just as clueless.

[23] *On Ancient Medicine* ch. 2, *CMG* I,1: 37.9ff.

[24] All who have tried to speak or write about medicine, assuming a postulate for their discussion, hot, cold, wet, dry or anything else they fancy, narrowing down the causal principle of human diseases and death making it the same for all, postulating just one or two principles, are evidently mistaken in many of their statements, and are particularly open to censure since they are talking about an art, which everyone uses on the most important occasions and where they especially honour good practitioners and craftsmen . . . So I have considered it to need no new-fangled postulate, as do obscure matters, about which anyone who speaks has to use a postulate, for example about things in the heavens or under the earth. If anyone were to speak and declare the nature of those things, it would not be clear either to the speaker himself or to his audience whether what was said was true or not, since there is no criterion to which one should refer to obtain clear knowledge. (*On Ancient Medicine* ch. 1, *CMG* I,1: 36.2–21)

But he then goes on to worry that 'the art as a whole comes in for much abuse from ordinary people, so that it is thought that there is no art of medicine at all'. The trouble is that doctors disagree about the treatments to be used: so that ordinary people are likely to object that this art resembles divination. 'For diviners too think that the same bird is a good omen if it is on the left, but a bad omen if it is on the right, while other diviners think the opposite.'[25]

This is an extraordinary remark from several points of view, and it provides me with an opening to probe the question of just where, if anywhere, ancient doctors could distinguish themselves from diviners. First it would have been very surprising if doctors had all agreed on treatment, whether in particular cases or in general, and surely discussions on the subject may, sometimes at least, have been productive and useful. But then second divination was, in all our ancient civilizations, including in Greece, indeed including in some other Hippocratic writings, generally considered a highly respected art. Of course, individual diviners were often treated as figures of fun, or accused of being frauds, cheats, imposters (both points that apply to healers too), though in some of our texts the diviners have a way of getting the last laugh. Tiresias has plenty of abuse heaped on him by Oedipus, but turns out to have understood Oedipus better than he did himself.

Then the third remarkable point is this: both divination and medicine were in the business of offering predictions, in the latter case prognoses of the outcomes of diseases. The treatise *Prognostic* even recommends the practice of prognosis in terms that echo those used in divination.

> It seems to me to be an excellent thing for a doctor to practise foresight. If he discovers and declares in advance by the side of his patients [i.e. without their telling him] the present, the past and what will happen in future, and supplies what the sick have omitted in their accounts, he will be the more believed to understand the problems of the sick, so that people will confidently entrust themselves to the doctor . . . You will be blameless if you learn and declare beforehand those who will die and those who will recover.[26]

[25] *On Regimen in Acute Diseases* ch. 3, L. II 240.8ff. The author first cites the idea that birds on the left are good omens, when in practice the usual belief was that it was birds on the right that are—a cunning move presumably designed to increase the readers' doubts about the reliability of divination from birds.

[26] *Prognosis* ch. 1, L. II 110.2ff. The phrase about telling in advance 'the present, the past and what will happen in future' echoes what is said of the prophet Calchas in the *Iliad* 1:70; and cf. Hesiod, *Theogony* 38 on the Muses.

So one important aim of prognosis was to impress your clients, though the suggestion that the doctors will be blameless if they have predicted a fatal outcome seems optimistic. But the key question was how medical prognosis is to be distinguished from divination. Both were conjectural, both were admitted to be fallible. But both claimed some successes (though some put those down to luck) and both represented themselves as drawing on experience, generally encapsulated in authoritative texts. That was the aim of the extensive collections of individual case histories in both Greek and Chinese medicine, though that left wide open the question of just how to interpret the data the doctors so assiduously recorded, a problem which I shall take up in Chapter 3. Everything depended on whether the connections that were claimed between signs and outcomes were well grounded. For that there had to be more than merely symbolic links, but genuinely causal ones. Or so some argued.

The plot thickens if we consider that one articulate Greek *defender* of astrology thought he could justify it on just those—genuinely causal—grounds. Ptolemy was well aware that there were plenty of Greeks and Romans who thought astrology was fundamentally flawed (Cicero's *De Divinatione* sets out the debate between the traditional and the sceptical points of view).[27] Ptolemy acknowledges the fundamental difference between the two branches of the study of the heavens that he practises. Mathematical astronomy, set out in his *Syntaxis*, is, in principle, demonstrative, since it is based on the incontrovertible methods of arithmetic and geometry: his term for 'incontrovertible' is *anamphisbētētos*, providing a particularly clear example that that was his ideal (as I argued in Chapter 1). That even allows him to turn the tables on both Plato and Aristotle (whose taxonomy of disciplines Ptolemy had just cited). Where Plato had treated the mathematical sciences just as preliminary studies leading up to the supreme discipline of dialectic, that is philosophy, Ptolemy rates both 'physics' (the study of nature) and 'theology' (including what Aristotle called 'first philosophy') *below* mathematical astronomy, precisely because the first two studies are conjectural, theology because it studies the invisible, physics because it studies unstable

[27] See the recent discussions in Denyer (1985); Beard (1986); and Schofield (1986).

natures, while the last can be and is exact.[28] Plato and Aristotle would have turned in their graves.

But astrology, he concedes, is inferior and less self-sufficient, though it can draw on experience too. In the opening chapter of the *Tetrabiblos* he sets out to justify both its possibility and its usefulness.[29] On the first score he invokes the obvious effects of the sun and the moon, which surely show that what happens in the heavens influences what happens on earth (he knows that the tides are linked to the phases of the moon). The trouble was that as he extends the point to the influence of remote planets, the supposed sympathies and resonances get more and more tenuous, the argument relies more and more on what tradition has handed down, and the justification falls back on the claim that the predictions were sometimes (at least) fulfilled (as, of course, many of his contemporaries already believed), and if they weren't, that was the fault of the astrologer, not of the art. As for its usefulness, he argues that the foreknowledge it yields can help one to grasp what is appropriate for the 'temperament' of the body, and above all will be conducive to calm, well-being, and joy.

The situation as regards medicine exhibits important similarities, not least with regard first to the explanations that could be given for the successes that were claimed and second to the excuses for failure. Of course, some doctors had elaborate theories about how medical prognoses could be made, which will be on my agenda in Chapter 3. But we have good evidence that some Greek doctors focused on the

[28] 'From all this we concluded that the first two divisions of theoretical philosophy should be called guesswork rather than knowledge, theology because of its completely invisible and ungraspable nature, physics because of the unstable and unclear nature of matter: so there is no hope that philosophers will ever agree about them: only mathematics, if one attacks it critically, provides for those who practise it sure and unswerving knowledge, since the demonstration comes about through incontrovertible means, by arithmetic and geometry' (Ptolemy, *Syntaxis* I 1, Proem, Heiberg 1 6:11–21).

[29] Of the means of prediction through astronomy . . . two are the most important and valid. One, which is first both in order and in power, is that whereby we grasp the aspects of the movements of the sun, moon and stars in relation to each other and to the earth . . . [This] has been expounded to you in its own treatise [*Syntaxis*] as far as is possible in a demonstrative manner (*apodeiktikōs*). We shall now give an account of the second and less self-sufficient method . . . We shall try to examine briefly the measure of the possibility and the usefulness of such prognostication before offering detailed instruction in the subject. (Ptolemy, *Tetrabiblos* I ch. 1, 3.32ff., Hübner)

causal explanations they thought they could give in order to distance themselves from their rivals. The writer of *On the Sacred Disease* famously refutes those whom he calls 'purifiers' (*kathartai*), 'quacks' (*alazones*), 'vagabonds' (*agurtai*), and 'mages' or even magicians (*magoi*) for invoking unpredictable and arbitrary divine agencies in their accounts. He insists that the so-called sacred disease (epilepsy, we may say, according to his rather careful description) has its nature and its cause like any other. 'I do not believe that the sacred disease is any more divine or sacred than any other disease, but on the contrary, just as other diseases have a nature from which they arise, so this one has a nature (*phusis*) and a definite cause (*prophasis*)' (*On the Sacred Disease* ch. 1 para 2, Grensemann).

That is a bold claim, repeated at the end of the work (ch. 18), where he goes even further: 'The so-called "sacred disease" is due to the same causes as all other diseases . . . Each disease has its own nature and power.' Indeed, 'there is nothing in any disease that is unintelligible or which is insusceptible to treatment'. So he even claims that most diseases can be cured, provided one catches them early enough, by adjustments to regimen. 'A man with the knowledge of how to produce by means of a regimen dryness and moistness, cold and heat in the human body, could cure this disease too provided that he could distinguish the right moment for the applications of the remedies' (*On the Sacred Disease* ch. 18, paras 1ff. Grensemann). Yet when it comes to outlining his own ideas about its cause, he says it comes about as a result of the vessels in the brain being blocked by phlegm. A cynic might see this as almost as much an article of faith as the beliefs of the purifiers he attacks, except that *in principle* his causes were regular and predictable, not arbitrary and wilful.

Greek doctors often in fact admit that they were at a loss to determine the causes of the diseases they encountered. Theories based on humours, on opposites, on elements, on repletion and depletion were bandied back and forth. In the Hellenistic period and later this produced something of a reaction, where debate and dispute came to focus not so much on this or that pathological theory but on methodology and aims in general. The Dogmatists, such as Galen (though he would have rejected the label), claimed that they could understand the causes of diseases and produce cures: but others lowered their sights on the first aim and abandoned the search for invisible entities and hidden causes as hopeless, in order to concentrate on what produced a cure (they hoped), or at least secured some relief. In his history of medicine (1st century CE), Celsus reports the Empiricists as arguing

> there is no need to inquire into how we breathe, but what relieves difficult and laboured breathing, nor into what may move the blood vessels, but what the various types of movement signify . . . A man of few words who learnt by practice to discern well would make an altogether better doctor than one who, unpractised, cultivates his tongue too much.[30]

Such knowledge is to be built up by experience, and theory is pointless.

Yet Galen himself thought he was on solid ground with some of his explanations. He feels confident enough to challenge some of his rivals to public debate on questions he thought could be resolved by experimental dissection. On one occasion he tackles an opponent who endorses Erasistratus's view that in their normal state the arteries contain only air, not blood. The blood that flows from a cut artery comes from neighbouring veins through 'capillaries', *sunanastomōseis*. This Erasistratean 'was always promising to exhibit the great artery empty of blood, but never did so. When some hot-headed youths brought animals to him and challenged him to show that, he declared he would not do it without a fee.' Thereupon Galen's supporters laid down 1,000 drachmae for him to pocket should he succeed. The outcome was a real botched job.

> In his embarrassment he made many twists and turns, but compelled by all those present he mustered courage to take a lancet and cut along the left side of the thorax where, he thought, once the perforation had been made, the aorta should become visible. He proved so little practised in dissection that he made the cut on the bone. (*On Anatomical Procedures* VII ch. 16, K. II 642.3–14)

I shall have more to say about Galen's own anatomical demonstrations in the next chapter.

So let me now draw some threads together and conclude. More or less formal debates, on all sorts of topics, are, as I noted at the outset, widely attested and certainly not confined to literate societies, ancient or modern. Insofar as human reasoning can be construed as essentially social, debate is one of its primordial manifestations, though as our study of antiquity shows, those manifestations exhibit considerable variation. The recurrent problem, to which of course there is no solution, is that the outcome is often influenced not just by the strength of the arguments on either side, but by extraneous considerations, including overtly political ones, the open or hidden agenda,

30 Celsus, *On Medicine* I Proem para 39, *CML* I 23.20–7.

or just the fads or fancies, of those who did the adjudicating, who might be the ruler, his ministers, or come to that a lay audience. Ancient persuaders usually had a double task, not just to put their case, but to educate their audience to recognize its strength. But then that is always the problem with genuinely innovative ideas.

Yet as we have seen, ancient debates could lead to fruitful reflections on second-order issues, the nature, methods, and aims of each field of research. Controversy could lead to greater self-awareness on those issues and a greater self-confidence in the pursuit of those aims. But there was also much arid confrontation—and that is not just an anachronistic modern reaction but a verdict some of the ancients themselves delivered. Moreover, disputes over fundamentals often revealed the vulnerabilities of the inquiry and of those who posed as authorities in it.

Some might conclude that the arguments I have reviewed are just a sign of the immaturity of the disciplines in question, their failure to achieve the status of what Kuhn called 'normal science'. That seems to me to be at best a half-truth. Some ancient inquiries suffered from the opposite weakness, of being too firmly established, when the existing authorities controlled access by insisting on long apprenticeship and the mastery of canons—as was the case with Mesopotamian scribes, Chinese functionaries, and many Greek doctors down the ages. Literacy was (as I shall be observing in Chapter 5) a two-edged sword. No doubt a degree of specialization and professionalization is needed to consolidate a discipline, but the risk then is that an elite will close ranks in the face of challenges whether from within the discipline or from outside it. Some ancient debates, as we saw, and not just between gurus, were merely esoteric and served to increase mystification rather than to dispel it. Meanwhile, at the opposite end of the spectrum, the populist alternative certainly put a premium on transparency but assumed that anyone should be able to adjudicate on any question, however technical.

If pioneering science is always going to be a matter for experts, it should not lose sight of a certain obligation to give an account of itself, and not just on intellectual matters. I shall be returning to the fundamental question of the values that underpin ancient investigations in Chapter 4. But before that my next main task will be to say more about ancient heuristics, where I shall explore how self-conscious programmes of empirical inquiry were developed, including in the teeth of doubts and objections, and the difficulties some innovators experienced in getting their discoveries accepted.

3

Heuristics and Its Hazards

In Chapter 1, I discussed the political background to the development of the concept and the practice of a powerful model of demonstration that certain ancient Greeks set their sights on, though it was one, so I argued, that had some negative as well as positive consequences, notably in that it sometimes distracted attention from heuristics. So I turn now to the topic of heuristics together with some of the hazards or the difficulties it faces. Let me explain the main issue in that regard with a basic example. According to a once standard but now outdated view, progress in science depends in the first instance on effective empirical research or data collection, leading to conjectures, theories, hypotheses that are then verified against further data and modified, if need be, generating new theories, to be tested in their turn in a gradual cumulative approximation to the truth.

That view was comprehensively abandoned even before the major upheavals in the philosophy of science in the 1960s, one of the main features of which was the recognition that there is no such thing as a pure observation statement. No observation is totally innocent: all presuppose a theoretical framework, so all are, as the jargon has it, theory laden. But that view in its turn is far from trouble free. If all observations are theory laden, how can any observation challenge the theories that it presupposes? How can the grip of an existing conceptual framework ever be loosened? Of course, the degree of theory ladenness can and does vary: some observations have more of a load than others (though we can never retreat to ones where the load is zero), and this leaves room for the possibility of the revision of concepts that lie at the periphery of some overall scheme, and even eventually of those that constitute its core.

These rustic philosophical musings prompt a series of questions as to how ancient investigators got on. In Chapter 2, I discussed how they

coped with the interpersonal tasks of persuasion in debate. Here I shall focus on their ideas of what is there to be investigated, and specifically on how to go about discovery there. How far were they aware of a need for, of the power of, and the difficulties in empirical research? How far did they devise tactics or techniques to deal with apparently intractable problems, where one topic we can explore is their use of idealizations or simplifications. Again did they show some sense of a need to improve the observational and other instruments available and to check the accuracy of those they had? What kinds of discovery did ancient empirical research generate and how indeed did the innovators persuade their contemporaries that they were indeed discoveries? How far did they appreciate the need to test theories—where we face the controversial topic of experimentation in antiquity, the believed lack of which has regularly been identified as the fundamental flaw in all of what passes as ancient 'science'? As before, my aim is not to assess the ancients' performance against ours, but to examine their procedures and ambitions against their starting assumptions, the constraints under which they worked, and in the light of the broad sweep of the history of human reasoning. Of course, humans have always been curious; but to suggest you need methods of finding out is another matter, possibly quite an aggressive move in that it may suggest you know better than your peers how to do it.

So my first task is to ask what types of hands-on research ancient investigators advocated, with what kinds of institutional help or hindrance, with what aims and success, and what intellectual justification. One method that was definitely hands on, messy, and controversial was dissection. Although much of its ancient history is well known (e.g. Kuriyama 1999 discussing both Greece and China), I believe there are still facets of it that are under-explored, and so I aim to make the most of what it can tell us about ancient heuristics. A naïve positivist view might be that, once such a technique had been shown to deliver results in the form of knowledge of internal anatomy, it would forthwith be universally adopted as standard procedure. Our evidence shows otherwise, and its very controversiality has an advantage, from the perspective of my inquiry here, as it will allow me to explore certain aspects of ancient methodological disagreements. As usual I shall range across several ancient cultures in my attempt to reveal the complexity of the issues, and in particular to help get the better-known Greek data into perspective.

But to make a start, and to reassure ourselves how seriously the need for research was sometimes taken, I shall begin with a Greek, the 3rd-century-BCE Alexandrian anatomist, Erasistratus, whose statement on that subject is quoted by Galen, *On Habits* 1 (*Scripta Minora* II, Müller) 17.11ff:

> Those who are completely unused to inquiry are, in the first exercise of their mind, blinded and dazed and straightway leave off the inquiry from mental fatigue and an incapacity that is no less than that of those who enter races without being used to them. But the person who is used to inquiry tries every possible loophole as he conducts his mental search and turns in many directions, and so far from giving up the inquiry in the space of a day, does not cease his search throughout his whole life. Directing his attention to one idea after another that is germane to what is being investigated, he presses on until he arrives at his goal.

The people Erasistratus has in mind are to be athletes of investigation, who should cultivate an intellectual muscularity.[1]

Erasistratus does not specify what kind of research he had in mind in that passage. But he was one of the two Greeks (the other is Herophilus) who are reported to have practised dissection and vivisection, not just on animals, but also on humans. But they were not the first Greeks to cut up animals in the name of research (as opposed to butchery) and so we must start further back. With the exception of the treatise called *On the Heart* (which is, in any case, nowadays thought to be contemporary with Herophilus and Erasistratus, e.g. Lonie 1973), there are no signs of systematic dissection in the Hippocratic Corpus. The first great advocate and practitioner of the method was Aristotle, which illuminates a rather different side to his work from the one I discussed in Chapter 1. These are visual demonstrations, not axiomatic-deductive ones.

Yet Aristotle does occasionally refer to his predecessors in this context. When he deals with earlier views on the blood-vascular system, he has this to say (*History of Animals* 511b13–23):

[1] Such views are echoed in many later texts. My colleague, Professor Nick Humphrey, draws my attention to a particularly striking parallel from Pavlov's 'Last Testament to the Youth of my Country', which emphasizes the qualities of consistency, patience, modesty, and passion needed in the researcher. 'Never think that you already know everything . . . Remember that science requires your whole life . . . Science demands of man the utmost effort and supreme passion' (Pavlov 1955: 54–5; cf. Babkin 1951: 110).

> The reason for their ignorance is the difficulty of carrying out observations. For in dead animals the nature of the most important blood vessels is unclear because they especially collapse immediately the blood leaves them . . . And in living animals it is impossible to investigate the nature of the blood vessels because they are internal. And so those who have examined dead bodies by dissection have not observed the principal sources [of the blood vessels], while those who have examined very emaciated living men have inferred the sources of the blood vessels from what could then be seen externally.

Aristotle's own preferred method was to use emaciated animals that had been strangled and then dissected. There is no question, then, for him, of post mortem dissection of a human body. Moreover, the extensive apologia for dissection in *On the Parts of Animals* (645a15ff., 28ff.) throws light on the inhibitions Aristotle had to overcome among his contemporaries:

> We must not feel a childish disgust at the investigation of the meaner animals. For there is something of the marvellous in all natural things . . . It is not possible to look at the constituent parts of human beings [understand, just as much as the parts of 'meaner animals'], such as blood, flesh, bones, blood vessels and the like, without considerable distaste. But when we discuss any one of the parts or structures, we must not suppose that what we are talking about—and the object for which our inquiry is undertaken—is their material, but rather the whole form, just as in discussing a house, it is the whole form, not the bricks and mortar and timber in themselves, that is our concern.

Remember, Aristotle had been a pupil of Plato for twenty years. Plato thought that anyone who contemplated particulars, rather than the intelligible Forms, was seriously mistaken as to the source of proper understanding—and it would be even more perverse to spend time cutting them up. To win over his fellow-Platonists, or at least not to be laughed out of court by them, Aristotle says that the objects of *his* inquiry too were forms, though these are instantiated in, not separated from, the perceptible particulars he had in front of him. His second powerful argument was that what his dissections revealed is the final causes of the parts of animals, the good they serve, in short the beauty of nature. You could not appreciate that unless you got out

your scalpel, overcame your disgust, and dissected. On the matter of disgust, we must remember that there were no effective antiseptics in Aristotle's day, nor any reliable anaesthetics that could be used in surgical procedures.

But Aristotle's investigations were on animals, which he also often vivisected. He seems to have had a strange predilection for removing the legs or wings of insects, but he would have said that this was to investigate how they moved.[2] Nevertheless, humans were still definitely off limits. So when it came to those Alexandrians I referred to, we start a new phase in the story, one with massive implications for ethics as well as for scientific methodology. Our main source is Celsus in the mid 1st century CE. In the Proem to book 1 of his *History of Medicine* he sets out the debate between three rival medical sects, two of which, the so-called Empiricists and the Methodists, were dead against dissection and vivisection. First, this was on ethical grounds (the cruelty that human vivisection implies: cruelty to animals is not mentioned, though Galen was to remark on the screams of vivisected animals and on the agonized look on the face of a vivisected ape).[3] But then there were also methodological objections. Human dissection is pointless, they argued, since the investigation of the dead body could tell you nothing that is relevant to how the living human being functioned. In the background were deep-seated inhibitions about the desecration of the human corpse, though that point should not be exaggerated since in practice dead bodies were not infrequently mutilated in a spirit of vindictiveness in the aftermath of battle. Meanwhile, if you used animal subjects for dissection and vivisection, the problem there was that it only told you about those animals, and the results could not simply be transferred to humans. That was a point that Galen was well aware of, despite Vesalius's later criticisms of him on just that score.

Those who were against dissection argued that doctors could get all the anatomical knowledge they needed in the course of their medical practice, for example from observation of wounded gladiators or

[2] E.g. *History of Animals* 519a27ff.; *Progression of Animals* 708b4ff.

[3] In *On Anatomical Procedures* IX ch. 11 (Duckworth 1962: 15), Galen suggests using pigs or goats as subjects for his work exposing the brain of living animals, to avoid having to look at the face of an ape when vivisected. At *On Anatomical Procedures* VIII ch. 3, K. II 663.11ff., he chooses pigs in his investigations of the vocal cord precisely because their screams make the demonstration so much more dramatic: see below, p. 85.

soldiers. Celsus (*On Medicine* I Proem para 43, *CML* I 24.12–18) reports the Empiricists as arguing:

> It follows that the doctor just cruelly plays the cut-throat, and does not learn what our viscera are like when we are alive. If, however, there be anything to be seen while a man is still breathing, chance often presents it to the view of those treating him. For sometimes a gladiator in the arena or a soldier in battle or a traveller set upon by robbers is so wounded that one or other internal part is exposed . . .

On the other side, the Dogmatist advocates of the method are reported as arguing that

> Herophilus and Erasistratus proceeded in by far the best way: they cut open living men, criminals they obtained out of prison from the kings—and they observed while their subjects still breathed, parts that nature had previously hidden, their position, colour, shape, size, arrangement, hardness, softness, smoothness, points of contact, and finally the processes and recesses of each and whether any part is inserted into another or receives the part of another into itself. (21.15–21)

They were evidently aware that they were open to the charges of cruelty and immorality, but they countered those with an argument that the end justified the means: 'nor is it cruel, as most people state, to seek remedies for multitudes of innocent men of all future ages by sacrificing only a small number of criminals' (21.29–32, where it is recognized that 'most people' did, in fact, think it cruel).

Of course, Herophilus and Erasistratus could not have done what they did without the support of the state authorities, and that marks the crucial difference between them and Galen, for by his time, in the 2nd century CE, there was no question of human vivisection, and even human dissection was rare and difficult. You should go to Alexandria, he said,[4] to study human osteology, since it is still possible there. He

[4] *On Anatomical Procedures* I ch. 2 (K. II 220.11ff):

> Make it rather your serious business not only to learn accurately the shape of each bone from books, but also to examine assiduously with your own eyes the human bones themselves. This is quite easy at Alexandria since the doctors there instruct their students in this matter by means of demonstrations before their very eyes. For this reason, if for no other, try to visit Alexandria.

Galen frequently underlines the need for practice. 'You should not delegate the work to assistants' (e.g. *On Anatomical Procedures* I ch. 3, K. II 232.18ff): 'you yourself have to learn how to cope with the difficulties the procedure presents, for example in the control of haemorrhage' (VII ch. 12, K. II 628.13ff.).

says it is 'quite easy' to do so, though that is probably optimistic, and more importantly this is just limited to the study of the bones. Otherwise his own direct experience of human subjects for dissection depended on chance. On one occasion he found a corpse exposed when a river washed away a tomb; on another an unburied robber—not exactly favourable circumstances to investigate human anatomy, even though he says it was as if the skeleton were laid out for dissection.[5]

Galen offers us our most extensive ancient Greek recommendation for the dissection (and vivisection) of animals, and like Aristotle, he puts considerable emphasis on the teleological motivation, the investigation of forms and final causes and the exploration of the beauty of nature. In fact, three of the four justifications for dissection he proposes all refer to causal knowledge.

> Anatomical study has one use for the student of nature, who loves knowledge for its own sake, another for him who values it not for its own sake, but rather to exhibit that nature does nothing without an aim, a third for one who provides himself from anatomy with data for investigating a function, physical or mental. (*On Anatomical Procedures* II ch. 2, K. II 286.3–8)

Yet even more important, in his view, is a fourth, practical, aim (283.12–284.11):

> What could be more useful to a doctor for the treatment of war wounds, for extraction of missiles . . . than to know accurately all the parts of the arms and legs and . . . all the external parts of the shoulders and back, breast and ribs, abdomen, neck and head . . . If a

[5] *On Anatomical Procedures* I ch. 2, K. II 221.1ff:

> But if you cannot [go to Alexandria], it is not impossible to see something of human bones. I, at least, have done so often when a grave or a monument was broken open. Thus once a river, inundating a grave that had been hastily made some months previously, easily broke it up, and completely swept away the body of the dead man by the force of its movement. The flesh had already putrefied, though the bones still maintained their proper relations to one another . . . This skeleton was as though a doctor had deliberately prepared it to instruct some youth. And on another occasion we saw the skeleton of a robber, lying on rising ground a little off the road. He had been killed by some traveller repelling his attack. None of the inhabitants would bury him, but in their hatred of him were glad enough to see his body consumed by the birds, which, in a couple of days, ate his flesh, leaving the skeleton as if laid out for teaching purposes.

At *On Anatomical Procedures* III ch. 5, K. II 385.5–8, he reports that dissections were carried out on a German enemy killed in battle, though the lack of experience of those who conducted them meant they learnt no more than just the position of the viscera.

man is ignorant of the position of a vital nerve, muscle, artery or important vein, he is more likely to be responsible for the death, than for the saving, of his patient.

So it turns out that the most useful part of the practice of dissection 'lies in just that exact study that is neglected by the professed experts', literally those who are clever at dissecting (*On Anatomical Procedures* II ch. 3, K. II 288.3ff). He means the theorists who quarrelled over such matters as how many valves there are at the base of each of the auricles and ventricles of the heart, and how many vessels communicate with each of them. That, to his mind, is less important, for practical purposes, than accurate knowledge of the nervous and blood-vascular systems. So he not only defends dissection, but recommends his ideas on the usefulness of the method against those of other practitioners, who do not appreciate its practical importance.

We may accept Galen's own account of those aims as far as it goes, but a factor that he does not mention in *On Anatomical Procedures*, but which must certainly also be taken into account, is that dissection could be used to build up a reputation and to defeat rivals. On several occasions, one involving the dissection of an elephant, no less,[6] Galen describes how he correctly predicted what would be found, in that case concerning the presence of a bone, the interventricular septum, in its heart—to the humiliation of his opponents. But note that that meant him sending one of his colleagues to plead with Caesar's cooks to allow him to extract the bone, which Galen kept as some kind of trophy of his victory. He was evidently a past master at exploiting the theatricality of competitive public dissections.[7] I have already underlined the

[6] A very large elephant was recently killed in Rome. Many doctors crowded to see it dissected and to learn whether the heart has two apexes or one and two cavities or three. Before it was dissected, I maintained that the same structure of the heart would be found in it as in all the animals that breathe air . . . But our inexperienced colleagues . . . supposed that the heart contains no bone, even in an elephant . . . However, when the heart was removed by Caesar's cooks, I sent one of my trained associates to ask the cooks to allow him to extract the bone from it. This was done and I have it to this day.' He even goes on, in a rare fit of generosity, to exonerate Aristotle for his mistakes, given his lack of experience in dissection. 'It is no surprise that Aristotle, among his many anatomical errors, thinks that the heart in large animals has three cavities. Since he lacked anatomical training, he failed to find the parts, so that is not surprising and he deserves to be forgiven. (*On Anatomical Procedures* VII ch. 10, K. II 619.16–621.7; cf. Scarborough 1985; Hankinson 1988)

[7] Cf. Manuli and Vegetti 1977; Vegetti 1979, 1983; von Staden 1995.

importance, in antiquity, of the need to be something of an exhibitionist, at least a self-publicist. In certain circumstances, indeed, that proves to be one of the main driving forces in heuristics.

We are dealing, as so often, with adversarial Greeks, where a technique that *might* have been limited to research and instruction was turned into a way of discomfiting rivals—in public, indeed. It is now time to turn to other civilizations to see what we can learn from them, and especially to China. A first fundamental point is that we should certainly not suppose that the same assumptions about the human body were entertained in China as those we find in Greece—nor that those assumptions were uniform in either culture. But if we may allow ourselves a very broad generalization, where most Greeks concentrated (though not exclusively, of course) on structures, static parts, and organs, the usual Chinese focus was far more on the processes occurring in the body. Health was often envisaged in terms of the unimpeded flow of *qi* (breath/energy) within the body, between viscera imagined as depots of *qi* of different kinds. Disease arose from a variety of causes but especially from the blockage of that flow and from the invasive effects of pathogenic *qi* (cf. Sivin 1987).

That does not mean that there was in ancient China no interest in the internal disposition of those viscera and the vessels that connected them and allowed their interactions. To practise acupuncture and moxibustion (burning dried moxa leaves applied to particular points on the patient's body), Chinese doctors had to know the paths of the *jing* (circulation tracts) and the *mai* or *mo*, the meridians and blood vessels within the body, in considerable detail indeed (Figure 3.1). Of course, sometimes that knowledge was a matter of what could be inferred from those actual practices—just as Greek ideas of the blood vessels were in part derived from what they thought the various effects of venesection told them.[8] The connections had to be there, since the effects claimed indicated they must be.

But we also have evidence of direct Chinese intervention on the human body, of Chinese dissection, in other words. One well-known text from the Chinese medical classic, the *Huangdi neijing lingshu* 12, from around the turn of the millennium, suggests the possibility of dissection but does not follow it up. In an exchange between the

[8] Cf. Lloyd 1991, ch. 8: 180 and n. 65.

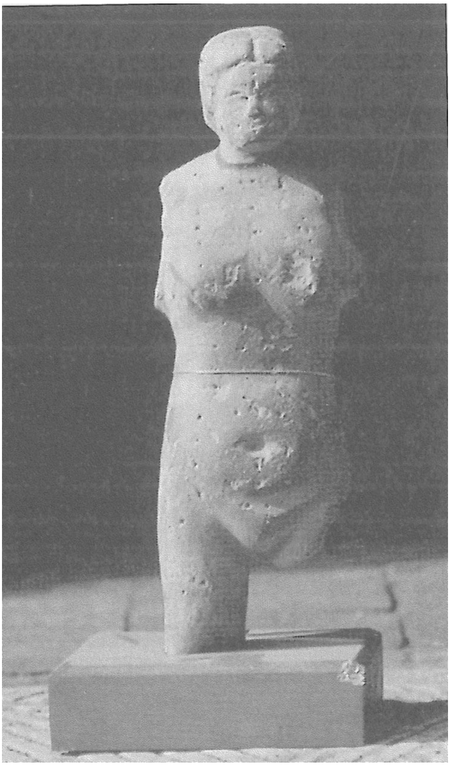


Figure 3.1 Model of acupuncture tracts. Most of the standard illustrations of acupuncture tracts date from the 16th century or later. Here is one that dates from the Eastern Han (before end of 2nd century CE) from the Zhang Zhongjing tomb in Nanyang, Henan

Yellow Emperor and the doctor Qi Bo, the latter speaks of what can be learned from 'cutting open and inspecting',[9] where, as in Aristotle's *On the Parts of Animals* (I ch. 5, 644b22ff.) there is an argument

[9] In *Huangdi neijing lingshu* 12, Qibo expostulates with the Yellow Emperor that much can be learned by 'cutting open and inspecting'. Whereas the height of the heavens and the breadth of the earth transcend what human beings can measure (contrast the *Zhoubi*, below, pp. 74–5) the human body is directly accessible. One can measure the exterior of the body and after death one can cut it open and examine it, to determine the consistency of the *zang* (*yin* visceral systems of function) and the size of the *fu* (*yang* visceral systems of function), the length of the vessels, the clarity or turbulence of the blood and its amount, which vessels have more blood and less *qi*, and which vessels the reverse. 'For all of these there are general numerical regularities (*da shu*)' (Yamada Keiji 1991: 40–1; cf. Kuriyama 1999: 155–7).

contrasting the difficulty of investigating the heavens with the fact that you can investigate and measure the human body directly. But were the techniques alluded to in that Chinese text ever carried out? The only passage from Han times (that is down to the end of the 2nd century CE) that provides circumstantial evidence to suggest an *actual* dissection comes not in a medical treatise, but in one of the universal histories. This is in the biography of the usurper Wang Mang in Ban Gu's *Han Shu*.

> Wangsun Qing, the confederate of Zhai Yi, was captured. Wang Mang sent his personal palace physician and others to work with skilled butchers to dissect Wangsun. They measured and weighed his five *zang* (*yin* visceral systems of function) and used a bamboo strip to trace the courses of the vessels (*mai/mo*) to learn where they began and ended. The Emperor said this knowledge could be used to cure illness.[10]

Quite how is not elaborated.

The circumstances on this occasion were evidently exceptional, the treatment of a prisoner of war. Part of the object, it has been suggested, was punitive, and maybe the covert message was the cruelty of Wang Mang. Elsewhere, we have an anecdote about the notorious tyrant Zhou Xin cutting open a pregnant woman to look at the fetus.[11] These texts may suggest that the rarity of references to dissection in Han times may not merely be an artefact of the limitations of our sources, but reflect moral disapproval of any such procedure. Much later we do find references to post mortem dissection in China, but this was for forensic purposes, to determine the cause of death.[12] The Greeks and Romans did not think of that. Conversely one motivation we find in Greece, to show that nature does nothing in vain, had no parallel in China, and that strengthens the possibility that the controversy between teleologists and their opponents in Greece may have acted as a key stimulus, on the former, to undertake detailed anatomical research to support their cause. In both China and Greece what dissection was *for* was evidently crucial. Heuristics, as I said, has to be anchored to assumptions, some of which were open to challenge, others rather beyond reach.

[10] *Han Shu* 99b, 4145–6, after Yamada Keiji 1991: 39f.; cf. Kuriyama 1999: 155.

[11] *Lüshi chunqiu* 23 4 1, which contains a list of the 'improper actions' of Zhou Xin. They also include cutting open the calf of a man who forded streams and was able to withstand great cold, in order to examine his marrow. From Knoblock and Riegel 2000: 596.

[12] Lloyd 1996a: 196–7.

These Greek dissections certainly delivered new anatomical knowledge, though how far that penetrated beyond a tiny circle of interested members of the elite is a moot point. Herophilus and Erasistratus identified the sensory and motor nerves as such, but confusion between them and other structures that had also been called *neura* in Greek and *nervi* in Latin—tendons and ligaments, for instance—was still common. But although, as Galen said, such knowledge was important for the surgeon, it was otherwise of little use in the treatment of illnesses. In that context, however, many doctors in both Greece and China set about assembling extensive databases, in the form of collections of individual case histories, starting with the Hippocratic *Epidemics* in Greece in the 5th century BCE and with the 2nd-century-BCE case histories of Chunyu Yi in China (recorded in his biography in the *Shiji*, as noted in Chapter 2). They certainly succeeded in providing the doctor with detailed records of past practice: but that left wide open the question of just how to interpret the data in question and how that was to be related to the new cases the doctors faced. Some Greek doctors stressed that every case was unique, which tended to the extreme conclusion that there are as many diseases as there are patients.[13] The problem was how to extract valid generalizations from the data. There was no algorithm for that: we still do not have one, despite all the efforts of those who advocate EBM (evidence-based medicine).

Of course, most aspiring Greek and Chinese doctors were taught a variety of techniques to diagnose complaints and predict outcomes. Initially the doctors represented in the Hippocratic Corpus made no use of the pulse as a diagnostic tool, relying mainly on a battery of other signs, varying from the famous Hippocratic face/*facies*, to what was revealed in the patient's stools, urine, and vomit. But from Praxagoras in the early 3rd century BCE onwards, pulse diagnostics came to be an essential part of the doctor's training. In the next generation Herophilus distinguished pulses according to 'magnitude', 'speed', 'intensity', 'rhythm', 'evenness', and 'regularity' (von Staden 1989: part VII, 262ff., and Texts 144–88). He understood that pulse rates vary with the age of the patient and he coined terms for particular abnormalities, the 'ant-like' and the 'gazelle-like' pulses among them. According to Marcellinus (*On Pulses* ch. 11, von Staden 1989 Text 182), he even designed a portable water clock which

[13] The author of the Hippocratic treatise *On Regimen in Acute Diseases* (ch. 1, L. II 228.2–6) criticizes some of his opponents on the grounds that their proliferation of the various types of diseases tends towards such a conclusion and so should be resisted.

could be calibrated to suit the age of a particular patient. Recognizing a correlation between the frequency of the pulse and body temperature, he inferred how feverish a patient was from the extent to which the pulse exceeded the normal rate for a person of his/her age. So the water clock acted not just as a timing device but as a primitive thermometer, though without any clear idea of degrees of heat (cf. von Staden 1989: 282–3).

In China, as Kuriyama (1999) and Hsu (2010) have both shown, pulse theory was possibly even more elaborate, reflecting Chinese ideas on the interacting processes in the body. Chunyu Yi was something of a specialist in the field and he was almost certainly an innovator, although he says that he was taught pulse lore from books and by his teachers (cf. above, Chapter 2, p. 47). In his detailed account of some twenty-odd patients he records that not all recovered, but even in the cases that ended in death, he claims that his prognoses, largely based on the pulse, had been correct. Hsu identifies some thirteen canonical pulse qualities in the *Mai Fa*, the first canon on the pulse on which Chunyu Yi draws, those she renders 'elongated', 'strung', 'coming', 'frequent', 'swift', 'leaving', 'difficult', 'sunken', 'floating', 'firm', 'tight', 'uneven', and 'not drumming' (Figure 3.2).

chang 長 – elongated (standard *Mai jing* pulse) *xian* 弦 – strung (standard *Mai jing* pulse)	case 1
given as: *[mai] chang er xian*	
lai 來 – to come (canonical pulse quality) *shuo* 數 – frequent (standard *Mai jing* pulse)	case 2
ji 疾 – swift (canonical pulse quality)	
qu 去 – to leave (canonical pulse quality)	
nan 難 – with difficulty (canonical term, not a pulse quality)	
bu yi 不一 – not one (no textual parallels except in *Shi ji)*	
given as: *[mai] lai shuo ji qu nan er bu yi*	
yin yang jiao 陰陽交 *yin* and *yang* intermingle (canonical term, not a pulse quality)	case 4
chen 沈 – sunken (standard *Mai jing* pulse), but here a body technique: sinking *fu* 浮 – floating (standard *Mai jing* pulse), but here a body technique: floating	case 5
jian 堅 – firm (canonical pulse quality)	
jin 緊 – tight (standard *Mai jing* pulse)	
given as: *chen zhi er da jian, fu zhi er da jin*	
bu ping 不平 – uneven (*ping*: canonical pulse quality) *bu gu* 不鼓 – not drumming (*gu*: canonical pulse quality)	case 6

Figure 3.2 Pulse qualities in the *Mai Fa* or canon of pulses mentioned in the first six case histories of Chunyu Yi (*Shiji* ch. 105)

This was, for sure, a great parade of expertise—showing off again. Apprentice doctors, whether Chinese or Greek, had first to learn to identify what it was they felt: and then they had to correlate the pulse in question with a pathological condition and with a possible outcome. Of course, such modes of divination as hepatoscopy, the study of the marks on the liver of a sacrificial victim, could be almost as elaborate and learned. In both cases, everything depended on whether the connections that were claimed between signs and outcomes were well grounded. For that there should be more than merely symbolic links, but real causal ones, though many settled just for regular correlations. That is the crucial issue, as we found before (Chapter 2, p. 52) when discussing Ptolemy's defence of astrology.

Usually the signs just revealed effects, and the process of reasoning was from them back to causes. Some Greek doctors certainly realized that similar effects could arise from different causes. Theophrastus was to raise the question of whether the same effect always comes from the same cause, or whether it can proceed from different causes (*History of Plants* IX 19.4). But already the writer of *On Regimen in Acute Diseases* (ch. 11, L. II 314.12ff.) criticized certain doctors who failed to distinguish correctly between the different possible causes of the weakness of a patient. That could be the result of starvation, or of some other irritation, or because the patient was in pain, or again from the acuteness of the disease. Nor do they differentiate properly the different kinds of affections that arise from our nature and our habits, and yet 'knowledge of such matters brings recovery and ignorance death' (316.5–6). Again Aristotle explicitly drew attention to the problem that inference to a cause from some signs is insecure. In his *Rhetoric* (1357b18–21), one example is a diagnosis that someone has a fever from the fact that his breathing was difficult. In the *Prior Analytics* (70a21, 36ff.) he points out that a sallow complexion may but often does not indicate that a woman is pregnant. So the problem of inference from signs was to differentiate those that did, and those that did not, provide conclusive evidence of their causes (cf. Burnyeat 1994; Lloyd 2007b).

From other fields too, some ancient examples of theory infiltrating what it was claimed could be *seen* are startling. The confirmation bias could be and often was at work even at the level of what is said to be perceived, let alone what was said to be inferred. Aristotle says that we *see* that what brings about a movement is in all cases either in contact

or continuous with what it moves,[14] and again that we *see* that differences in the speed of falling objects depend either on the medium traversed or the weight of the object.[15] One of his arguments to show that the earth is at rest in the centre of the cosmos begins with the statement that 'heavy bodies are borne towards the centre of the earth' (*On the Heavens* 296b18ff.). He goes on to cite as an indication (*sēmeion*) of this that weights moving towards the earth do not move in parallel lines but at the same angles to it, so that they are moving towards the same centre, that is the centre of the earth. He certainly did not observe their non-parallel fall: he simply assumed it.

It is true that he also has enough genuine empirical grounds for his theory that the earth is spherical: first, the shape of the earth's shadow in a lunar eclipse (*On the Heavens* 297b24ff.); and, second, the observed changes in the visibility of circumpolar stars as one moves southwards.

> The appearances of the stars, [he says (297b30ff.)] make it clear not only that [the earth] is spherical, but also that it is of no great size. For a slight change of position on our part to the south or to the north is accompanied by a noticeable difference in the circle of the horizon, so that the stars above our heads change their position considerably, and the same ones are not seen by us as we move northwards or southwards. Some stars that are visible in Egypt and near Cyprus cannot be seen in northern lands and stars that are continuously visible in northern lands are seen to set in those regions.

Yet some who continued to believe that the earth is flat had objected that when the sun rises or sets, the line of the horizon *appears* straight, whereas if the earth were spherical, the horizon would necessarily be curved (294a1ff.). To that Aristotle in turn countered that that appearance is deceptive, 'for they do not take into account the distance of the sun from the earth or the size of the earth's circumference'. Given these assumptions, we have the makings of a research programme, one that sets out to determine the size of the earth. He is

[14] But if necessarily what brings about a movement according to the primary, spatial and corporeal mode, is either in contact or continuous with what is moved, as we *see* in every case, so necessarily things that move and their movers are continuous or they are in contact with one another, so that a unity is formed from them all. (Aristotle, *Physics* 242b59ff.)

[15] For we *see* that the same weight and body moves faster for two reasons, either because of a difference in the medium traversed—for example through water and earth, or through water and air—or, other things being equal, because of a difference in what is moved, in their greater weight or lightness. (Aristotle, *Physics* 215a25ff.)

even prepared to put a figure to the result, or rather to cite the figure the mathematicians 'who attempt to calculate the circumference' gave, namely 400,000 stades (*On the Heavens* 298a15ff.).

Aristotle does not actually record either the method those mathematicians used or how they arrived at that specific figure. But other sources report two later investigations.[16] In the 3rd century BCE, Eratosthenes is said to have based his calculation on observations of the shadow cast by a gnomon at noon on the day of the summer solstice at two points on the earth's surface, namely Alexandria and Syene, which he assumed to be on the same meridian (Figure 3.3). At Syene there was said to be no shadow, while at Alexandria there was one of a fiftieth of a circle, i.e. seven and one-fifth degrees. Taking the distance between the two locations to be 5,000 stades, Eratosthenes arrived by simple geometry at a figure of 250,000 stades for the circumference of the earth, which he then appears to have 'adjusted' to 252,000 stades to give a round number of stades for each sixtieth division of the circle.

There is a second example of an attack on the problem from the 1st century BC Posidonius, who used a method based on comparing

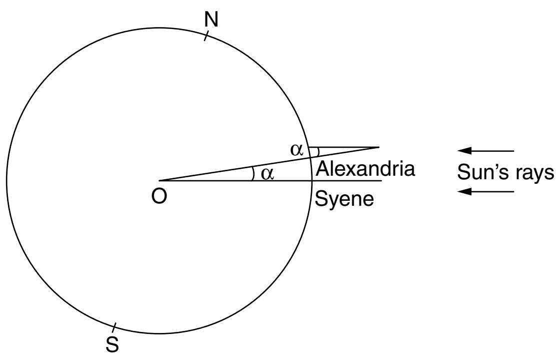


Figure 3.3 Eratosthenes's calculation of the circumference of the earth, based on gnomon shadow differences at Alexandria and Syene

[16] Our chief source is Cleomedes, *On the Circular Motion of the Heavenly Bodies* book 1, ch. 7, but there are further references to the problem, not always consistent with Cleomedes's reports, in Pliny, *Natural History* 2.247, and especially Strabo, 2 5 7, Bowen and Todd 2004: 84 n. 22.

observations of the star Canopus above the horizon at Rhodes and at Alexandria (Figure 3.4). Taking these two places to be 5,000 stades distant on the same meridian and the difference in altitude of Canopus to be 7½ degrees, he obtained a figure of 240,000 stades for the circumference of the earth.

There was a time when these famous procedures were greeted with uncritical enthusiasm and their weaknesses overlooked, but in recent years the elements of idealization and sheer guesswork they involved have come to be underlined (see especially Bowen and Todd 2004). How was the distance between Alexandria and Syene calculated (overland), or again that between Rhodes and Alexandria (by sea)? As for Alexandria and Syene being on the same meridian, in fact Syene is three degrees east of Alexandria, Rhodes about two degrees

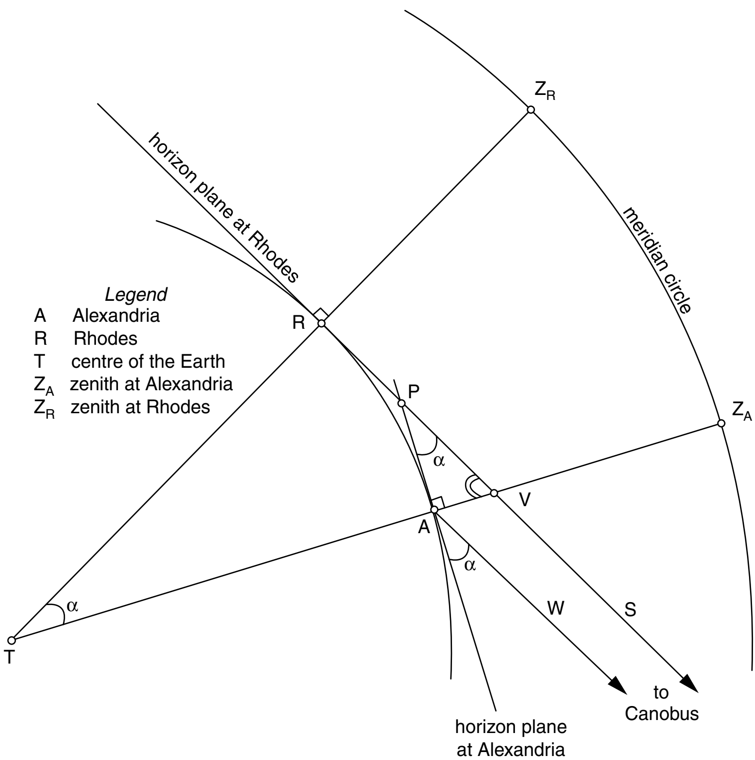


Figure 3.4 Posidonius's calculation of the circumference of the earth based on observations of the star Canopus from Rhodes and Alexandria

west. Then how exact, how accurate, were the observations of the noon shadows and of the height of Canopus above the horizon? In the first case, Cleomedes (1 7:75–76) records that the lack of a shadow at Syene was reported over a distance of 300 stades. Observation of celestial bodies at the horizon is (we should say) liable to particular distorting effects of refraction—a phenomenon of which Ptolemy for one was evidently aware (*Syntaxis* IX 2, Heiberg II 210.2ff.: refraction between different media is the object of a series of experiments in Ptolemy's *Optics*). Finally, in both cases we are faced with the problem that it is not certain what the value of the stade in question was (for several different stades were used in the Greek world). So for all these reasons some have concluded that these are purely and simply hypothetical thought experiments, not based on any actual observed values, though others concede that some observations do lie at their basis. Yet in a sense their hypothetical status makes them more interesting, rather than less, for my concerns, since it bears on their appreciation of a possible way of attacking the problems.

But one fundamental point we need to consider relates to the assumptions and idealizations in play. Both Greek investigations assume a spherical earth, taken to have been proved by Aristotle. But they also assume that the sun (in Eratosthenes's case) and Canopus (in that of Posidonius) are indefinitely far away, so that their light can be thought of as falling on earth in parallel lines. If those lines were actually parallel, that would put the heavenly bodies at an infinite distance, of course. In the analogous Chinese investigations, for which we have evidence notably in the *Zhoubi suanjing* from around the turn of the millennium, the target and the assumption are both different, though the similarity in the procedures lies in the shared bid to reduce the problems to simple geometry. The principal Chinese target is not the size of the earth, but the distance of the sun, and the assumption is not that heavenly bodies are indefinitely far away, but that the earth is flat.

The way the topic is introduced in the *Zhoubi* gives us important insight into the degree to which mathematical procedures were recognized to be the way to crack a whole series of apparently recalcitrant problems. It was not just some ancient Greeks, but also some Chinese, who saw mathematics as holding the key to solving many of the puzzles of the universe—a topic I shall be returning to in the next chapter. The Master, Chenzi, is questioned by his pupil Rong Fang.

Rong Fang says that he has recently heard something about Chenzi's *Dao*.

> Is it really true that your Way is able to comprehend the height and size of the sun, the [area] illuminated by its light, the amount of its daily motion, the figures for its greatest and least distances, the extent of human vision, the limits of the four poles, the lodges into which the stars are ordered and the length and breadth of heaven and earth?[17]

To which Chenzi replies, 'It is true.' Rong Fang asks for an explanation, but is sent away with: 'Your ability in mathematics is sufficient to understand such matters if you sincerely give reiterated thought to them.' When he fails, he is told he has not tried hard enough: 'You thought about it, but not deeply enough . . . this is a case of limited knowledge and insufficient spirit.' However, Chenzi finally relents and takes him through the procedures. The contrast between this text and Euclid is striking. The Chinese pupil is evidently expected not just to understand the result but to internalize the procedures. Euclid, by contrast, just overwhelms you with his results.

The calculation of the height of the sun at noon at the summer solstice uses gnomon shadow differences in ways similar to those reported for Eratosthenes. In the *Zhoubi* three eight-foot-high gnomons are used distant from one another 1,000 *li* (that is about 500 kilometres) on a north–south axis (Figure 3.5). Knowing that distance, and taking it that observations confirmed the common assumption that a distance of 1,000 *li* gives a difference of one Chinese inch in the shadow, and then crucially assuming further that the earth is flat, the height of the sun can be got by similar triangles. Both assumptions are (to our minds) well off target, and indeed the first one came to be criticized as incorrect by later Chinese commentators. The distance that yields a gnomon shadow difference of one inch, for an eight-foot-high gnomon, is not 1,000 *li* but 150 (Cullen 1996: 113–14). So again, as in the Greek investigations, we have to assume that this is at least in part a thought experiment, though here too it shows that the possibility of such an inquiry was entertained despite the practical difficulties of carrying it out. In fact, gnomon sightings are only useful on a much smaller scale, if you have a flat terrain to make observations to draw a bead on a not-too-distant height, such as a city wall, for instance. As with

[17] *Zhoubi suanjing* 23–4, translation after Cullen 1996: 176–7.

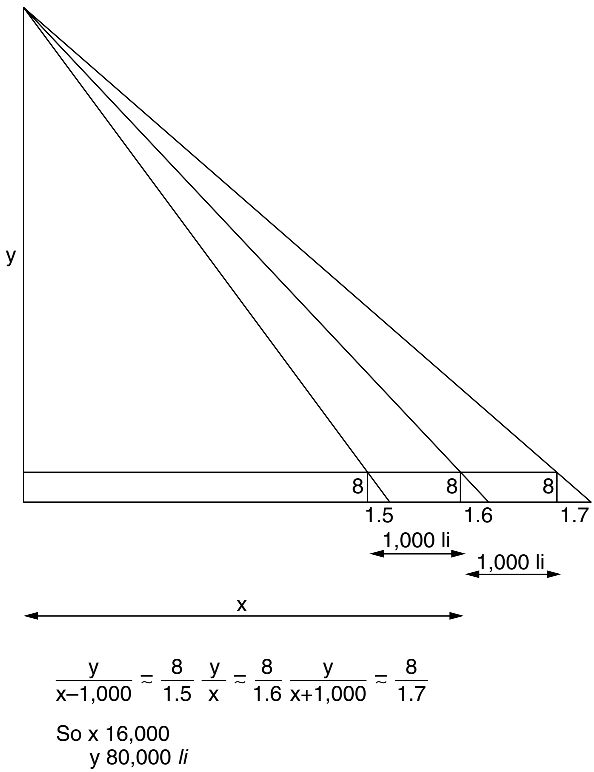
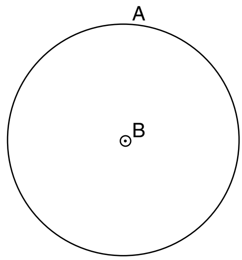


Figure 3.5 Gnomon shadow differences used to determine the height of the sun, assuming a flat earth, *Zhoubi suanjing*

the Greeks, they recognized that a problem situation might be idealized to be investigated: the question was when that helped, when indeed it was legitimate, when *they* thought it to be legitimate, that is.

Three Greek cases of the discounting of parallax illustrate the point dramatically (Figure 3.6). Aristotle already appreciated that those who observed the heavens from the surface of the earth were at some distance from its centre (which he took to be the centre of the universe). Ptolemy, following earlier astronomers, including Euclid, was confident that the earth itself could be treated as a point in relation to the sphere of the fixed stars (*Syntaxis* I.6, Heiberg I 20.5ff.). But in Aristarchus we find two much more controversial moves. In his treatise *On the Sizes and Distances of the Sun and Moon* (case 3 in Figure 3.6) he takes as one of his hypotheses that the



Case I: Ptolemy *Syntaxis* I.6: circle A = fixed stars;
circle B = the earth.
Case 2: Aristarchus' heliocentric theory: circle A = fixed stars;
circle B = circle in which the earth orbits the sun.
Case 3: Aristarchus' *On the Sizes and Distances:* circle A = moon's orbit round earth;
circle B = the earth.

Figure 3.6 Three cases of discounting parallax. In each case circle B is treated as a point

earth is as a point in relation to the moon's orbit. That effectively discounts lunar parallax, the differences that observation of the moon from different points on the earth's surface made, though these were considerable, as came to be pointed out by Ptolemy.[18]

But far more startlingly, in his heliocentric theory (for which our evidence comes from Archimedes, *Sand Reckoner* I 4, Heiberg-Stamatis II 218.7ff.) Aristarchus even proposed to treat the circle in which the earth orbits the sun as a point in relation to the fixed stars. This was evidently to counter the lack of any observed stellar parallax, for if the earth moves round the sun, the relations of the fixed stars when observed from different points on the earth's orbit would be expected to vary noticeably. Yet stellar parallax was not discovered until the work of Bessel and others in the 19th century. But recall that the assumption in both Eratosthenes's and Posidonius's determinations of the circumference of the earth was that the sun is at an indefinite distance from the earth. That certainly conflicted with the idea that the proposed earth's *orbit of the sun* could be discounted as effectively

[18] Thus Ptolemy was to discuss at length how to determine the allowance that has to be made for lunar parallax, *Syntaxis* V 11–12, Heiberg I 401.2ff., where he explicitly notes that the earth cannot be treated as a point in relation to the moon's orbit.

of zero magnitude in relation to the sphere of the fixed stars. Most Greek theorists assumed the sun to be indefinitely far away from the earth, while Aristarchus took it that the earth's orbit round the sun could be treated as a point in comparison to the sphere of the fixed stars—so indefinitely close!

The apparent lack of stellar parallax was one major sticking point that led to the rejection of heliocentricity. The other main one that Ptolemy mentions (*Syntaxis* I ch. 7, Heiberg I 21.9ff., 24.14ff.) is that the postulated daily rotation of the earth about its axis produced no visible effects at the earth's surface or on the clouds above us. If the earth rotated, you would expect the violence of that motion to have visible effects around us. Clouds, or missiles travelling through the air, could never move eastwards, for they would always be being anticipated by the motion of the earth itself. However, it appears from Ptolemy's own discussion that one possible counter to that objection had already been suggested, namely that it was not just the earth, but also the surrounding air, that rotates, a notion that was facilitated by the common Greek distinction between the lower air (*aēr*) and the upper (*aithēr*).

Yet Aristarchus's proposal found favour with only one other ancient astronomer, Seleucus of Seleucia, working around the middle of the 2nd century BCE. The two greatest names in 3rd- and 2nd-century-BCE Greek astronomy, Apollonius and Hipparchus, both retained the geocentric view (which had, in any case, to be the basis for an understanding of the moon's movements). Their programme focused on explaining the movements of the sun, moon, and planets by way of combinations of epicycles and eccentrics. Those models were only introduced after Aristarchus, and we have no evidence that he combined heliocentricity with them to produce a detailed (and it would have been simpler) set of explanations of the movements of the planets in particular. Yet the very suggestion that the dimension of the earth's orbit of the sun might be treated as negligible in comparison with the sphere of the fixed stars shows two things quite clearly: first the power of idealizations, including counter-intuitive ones, but also second, of course, their controversiality.

The heavens were, of course, the topic of intense interest, even of worship, from earliest times. The focus of that interest differed, to be sure, although both the regulation of the calendar (with more or less heavy political implications), and the bid to foretell the future were

common to all ancient civilizations. True, the instruments available for the sustained programmes of observation that were carried out were rudimentary. Many were carried out (as we have seen) with a simple gnomon or upright stick—a device that is widespread across the world and certainly not confined to literate civilizations. Yet both in China and in Greece we can document efforts to improve them. I mentioned one Chinese instance, Jia Kui and his ecliptic ring armillary, in Chapter 2, and a little later in the 2nd century CE a water-driven armillary was invented by Zhang Heng, though we are in no position to reconstruct it reliably (cf. Sivin 1995a: VI: 174ff.; Zhang Fan 2006: 477).

In Greece not only Ptolemy but also some of his commentators gave detailed descriptions of the instruments available to astronomers (cf. Dicks 1953–4 and Price 1957). To itemize these in ascending order of complexity, in the *Syntaxis* V 14 Heiberg I 417.1ff., Ptolemy describes the four-cubit dioptra that he says Hipparchus used (Figure 3.7). In V 12, Heiberg I 403.2ff., we have an account of the parallactic instrument Ptolemy used to determine lunar parallax and its zenith distance (Figure 3.8). In I 12, Heiberg I 66.5ff., there are the plinth (A in Figure 3.9) and the meridional armillary (B in Figure 3.9) used to determine the obliquity of the ecliptic, and in III 1, Heiberg I 196.21ff., the equatorial armillary or ring (C in Figure 3.9). Finally, in V 1, Heiberg I 350.14ff., there is an account of the construction and use of his most complex instrument, the armillary astrolabe (Figure 3.10). This consists of a nest of seven concentric rings with a pair of sights on the innermost ring. Once the instrument is set on a known fixed point (the sun, moon, or fixed star), it enabled the ecliptic

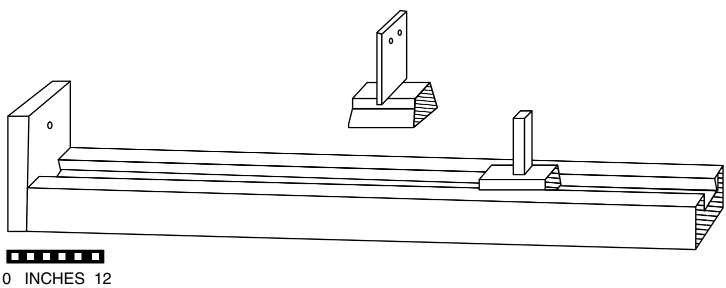


Figure 3.7 Hipparchus's four-cubit dioptra (from Ptolemy, *Syntaxis* V ch. 14)

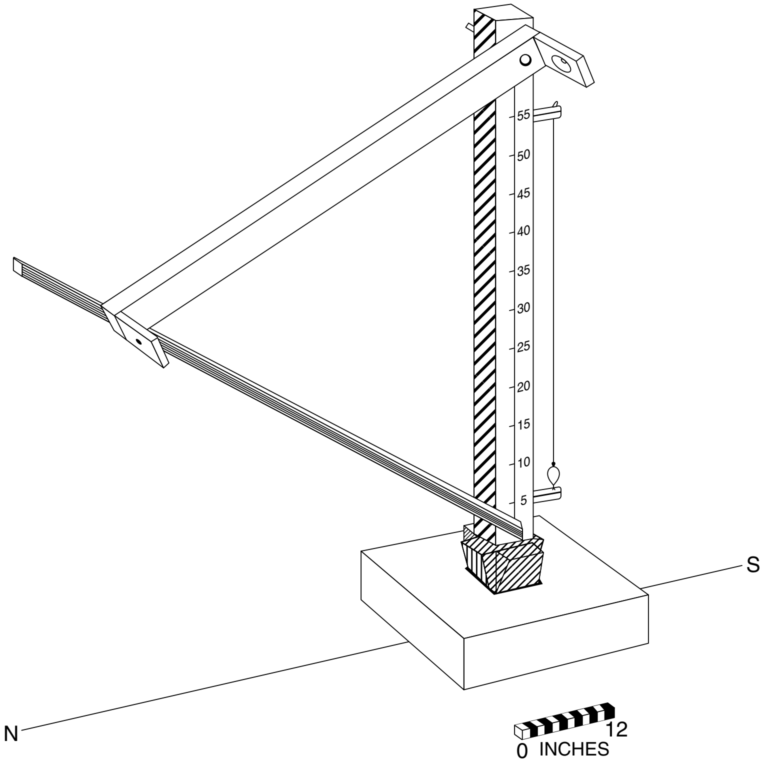


Figure 3.8 Ptolemy's parallactic instrument (*Syntaxis* V ch. 12)

coordinates of a heavenly body to be read off directly, rather than got by complicated calculations from observations of its position relative to the horizon and zenith. Ptolemy notes that the concentric rings must be accurately turned and that the convex outer surface of each of the inner rings should exactly fit the concave inner surface of the one above.

Interestingly, at III 1, Heiberg I 197.17ff., he remarks on a difficulty in the use of the equatorial armillaries. A deviation of a mere six minutes of arc from the equatorial plane generates an error of six hours in determining the time of the equinox, and he is particularly unhappy with the bronze rings that stood in the Palaestra at Alexandria, which are supposed to be in the plane of the equator. 'For so great is the distortion in their position, and especially in that of the bigger

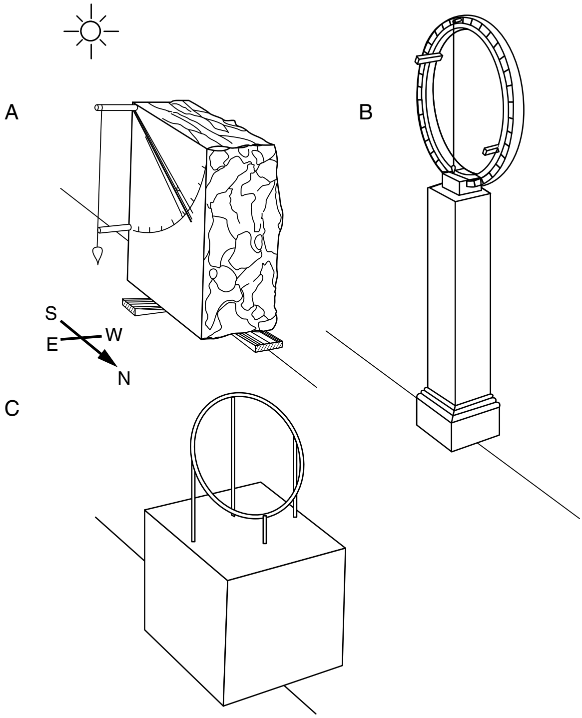


Figure 3.9 (A) Ptolemy's plinth (*Syntaxis* I ch. 12), (B) Ptolemy's meridional armillary (*Syntaxis* I ch. 12), and (C) Ptolemy's equatorial armillary or ring (*Syntaxis* III ch. 1)

and older one, when we make our observations, that sometimes their concave surfaces twice suffer a shift in lighting in the same equinoxes.'

This concern to provide accounts of the astronomical instruments used continues in such writers as Pappus and Theon of Alexandria (in the 4th century CE), and Proclus (in the 5th), who in some cases go beyond the description in the *Syntaxis* and afford us supplementary information on the instruments in question. Thus in his *Commentary on Books 5 and 6 of the Almagest* (Rome 1931, I. 4.4ff., 6.6ff.), Pappus remarks that Ptolemy did not specify the dimensions of the armillary astrolabe and gives a figure of a cubit for the largest, outermost ring. More surprisingly Proclus devotes several extended discussions in his *Outlines of Planetary Hypotheses* to detailed descriptions of the

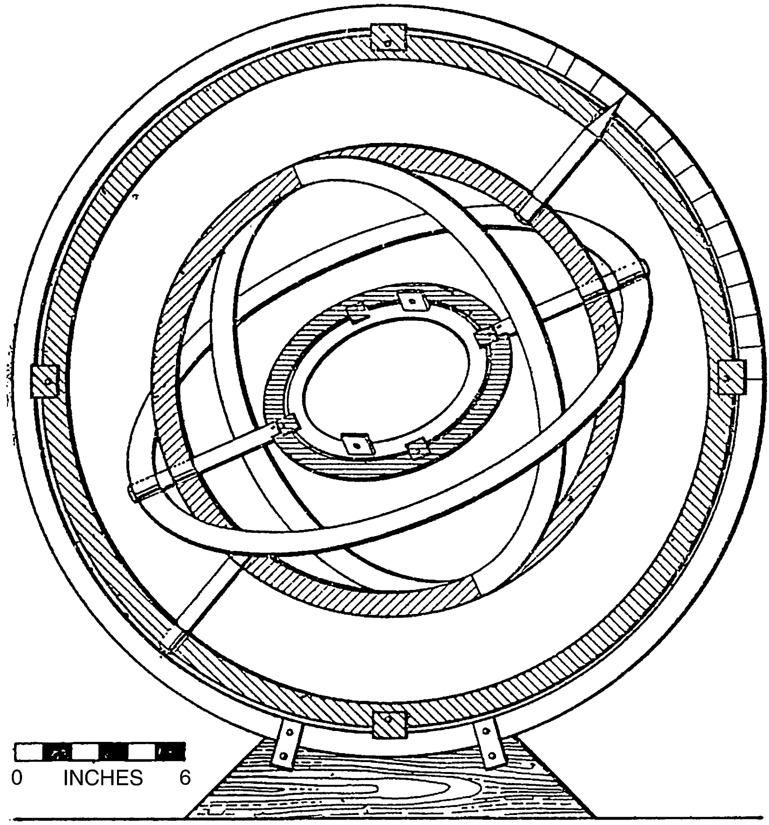


Figure 3.10 Ptolemy's armillary astrolabe (*Syntaxis* V ch. 1)

construction and use of astronomical instruments, and this despite the fact that as what he considered an orthodox Platonist he is often critical of the evidence of sense perception and so also of the reliability of the information that can be obtained by observation of the heavens.[19]

Of course, one recurrent problem in the astronomical observations the Greeks and Chinese carried out relates to accurate timekeeping. Again Ptolemy notices the problem (V 14, Heiberg I 416.20) and Pappus in his *Commentary* (Rome 1931, I. 87.1ff., 88.14ff) also says

[19] The chief passages are in ch. 3, 42.5–54.12, ch. 4, 128.6–130.26, and ch. 6, 198.15–212.6.

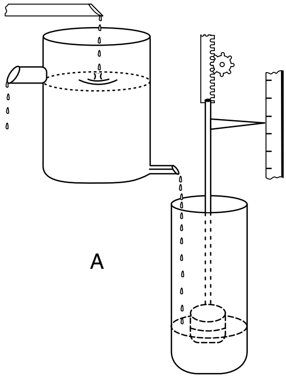


Figure 3.11 Ctesibius's constant-head water clock

that Hipparchus already rejected attempts to determine the angular diameter of the sun by using water clocks to measure the time it takes to rise at the equinox. Yet here too attempts to make improvements in timekeeping devices are attested in both Greece and China. The most notable Greek example is Ctesibius's construction of a constant-head water clock (Figure 3.11)—while in China, too, successive attempts were made, including for example by Zhang Heng, to tackle the same problem of controlling the variations in the water pressure in the water clock, in his case by introducing compensating overflow tanks (Needham 1965: 486).

The discoveries that the interest in the heavens led to vary. They include on the one hand the patterns of regularities in eclipse cycles and planetary visibility that the Mesopotamian scribes already knew about in the 7th century BCE (cf. Chapter 2, p. 35), and on the other, individual data such as the precession of the equinoxes, discovered by Hipparchus in Greece in the 2nd century BCE,[20] and rather later, by Yu Xi in the 4th century CE, in China. What those discoveries have in common was that the observers in question were not looking for what they eventually found. The Mesopotamian scribes, as we noted, were still scanning the heavens for signs. Hipparchus, according to the report

[20] Hipparchus's discovery is reported by Ptolemy, *Syntaxis* VII 2, Heiberg II 12.21ff.

For Hipparchus, in his work *On the Displacement of the Solsticial and Equinoctial Points*, adducing lunar eclipses from among those accurately observed by himself, and from those observed earlier by Timocharis, computes that the distance by which Spica is in advance of the autumn equinoctial point, is about 6° in his own time, but was about 8° in Timocharis's time. (Trans. after Toomer 1984: 327)

in Ptolemy, at first thought that precession just affected the zodiac circle. Thus his situation was both similar to and different from Archimedes in his mythical bath, different in that Archimedes had been set a problem, to determine whether the crown was gold or an alloy: but similar, in that in that story he is not represented as *planning* to get the answer by having a bath. He just wanted a bath and he would have then discovered the solution by spotting a connection.

Yet a striking feature of these astronomical discoveries may be their varied reception. When a predicted eclipse occurred, that showed that the scribes knew what they were talking about (though of course, some would always put down their success to luck). But while Ptolemy accepted Hipparchus's discovery of precession, in later Greco-Roman antiquity it was either ignored or flatly denied, even by some prominent members of the elite. Proclus in the 5th century CE, whose interest in astronomical instruments I have just mentioned, flatly denied the phenomenon in the *Outlines* (136.4ff. and 234.7ff.): for him the stars are fixed in the celestial vault that has no motion whatsoever other than the daily rotation.

A little later, in the 6th century, Philoponus implied that precession, like many another astronomical problem, was beyond comprehension. He first alludes to the different periods of revolution of the heavenly bodies and to the precession of the equinoxes at *On the Construction of the World* III ch. 4, 117.2ff., Reichardt, and then says

> Who would be able to state the causes of these things? No more could any human being give an account of the number of the stars, their position and order, and the difference in their magnitudes and colours. This only all believe, that God has made everything well and as is needed, neither more nor less. Altogether we know the causes of few things. If therefore people cannot give the natural cause of things that are apparent, they should not keep asking for the cause of things that are not apparent. (117.16–23)

Here then was an important discovery that was generally rejected simply because it conflicted with existing assumptions.

The heavens are not a possible object of experimentation. But my final topic, which I must deal with very rapidly, is to see how aware ancient investigators were of the need to test and verify their results. Of course, the experimental method, like the scientific method more generally, has, in modern times, been the subject of much rhetorical

exaggeration or hype, but as we know it today, it is in principle subject to standard procedures and controls.

It has often been claimed that it is the lack of experimentation that marks out all pre-modern 'science' (e.g. Thomson 1948: 94). Yet to that it should be said first that we should not underestimate the widespread role of ordinary, common or garden, trial-and-error procedures (at least) in many areas of human experience, especially in the development of technology. Although we usually cannot document the history of their use, we can hardly deny their importance in the development of agriculture, from the breeding of animals and the domestication of plants, to techniques of ploughing. It is true that a deep-seated conservativeness is often in evidence in the field of agriculture—understandable enough when we think that if your bright new ideas failed, you could jeopardize the very survival of your group. But that did not stop considerable innovations being made over the long span of the centuries, including in ancient civilizations, of course. The same is true, though with fewer elements of risk, in trying out new procedures in such fields as textile technology, of metallurgy, of pottery, and in many other domains. The problem in some cases related not to any reluctance to branch out but rather from the secretiveness of those who did, the need inventors often felt that they must keep their technologies to themselves, as was notably the case with the production and use of silk. Nor should we say that trying things out is limited to humans, for there is plenty of evidence from ethology to confirm that other animals are at it too.

Those points may be conceded, but what about *self-conscious* techniques of experimentation? Consider first Erasistratus's reported investigation of the invisible emanations that come from the living body, which at first sight has a certain modern ring about it. We do not have Erasistratus's own text, but the writer of the medical history preserved in a papyrus known as *Anonymus Londinensis* (77.45ff. Manetti) gives a brief summary. 'If one were to take an animal, such as a bird or something similar, and were to place it in a pot for some time without giving it any food, and then were to weigh it with the visible excrement that had been passed, he will find that there has been a great loss of weight. Clearly this has come about because there has been a considerable emanation which is only perceptible to reason.' Of course, the interpretation of the result assumes the theory (yet another example of the confirmation bias at work), but the

attempt to devise a test to throw light on the problem shows that this was a technique understood to carry some persuasive weight, as well as displaying the ingenuity of the experimenter.

Rather more detailed information of work that exhibits some of the features of experiment comes from Galen's investigation of physiological functions. In his *On Anatomical Procedures* he describes systematic dissections of the spinal cord at various levels to reveal the different effects produced. He is particularly proud of his investigations of the nerves controlling the vocal cord, which indeed he discovered,[21] another instance, like those I have mentioned before, where the theatricality of his demonstration is part of the object of the exercise. The spectators are astonished first when the animal loses its voice, and then even more astonished when the voice is restored when the ligature is removed.

Here, as in many other examples, experiments were designed to show a result or to prove a theory, rather than to evaluate alternatives neutrally. Experiments were witnesses and you generally only called those you thought would support you (Lloyd 1991: ch. 4). We do not find in any of our ancient civilizations a statement of the experimental method as such: but we do have repeated interventions, to refute an opponent, and to show the correctness of your own view, and though many were inconclusive, that did not stop them being deployed self-consciously as part of the rhetoric of persuasion. The very artificiality of the set-up contributed to its amazing character: yet that artificiality was sometimes used, by opponents, as an argument against accepting the results as applicable in normal circumstances.[22]

So what general conclusions can be suggested from the material I have passed under review? Many Greeks, Chinese, and Mesopotamians alike show considerable determination and ingenuity in pursuing programmes of empirical research. But heuristics faces problems:

[21] At *On Anatomical Procedures* IX ch. 13, he notes: 'When you come to the thoracic vertebrae, then the first thing that happens is that you see that the animal's respiration and voice have been damaged' (trans. Duckworth 1962: 24). This picks up his discussion in VIII ch. 4 (K 2 669.6–18):

> The animal cries out when struck, but it amazes the bystanders that it suddenly becomes silent when the nerves are tied with threads. They think it extraordinary that when small nerves in the back are tied, the voice is destroyed . . . If you want to loosen them again straight away to show how the animal recovers its voice—for this surprises the spectators even more—put loops on the knots and tighten them moderately, for such a loop will enable you to loosen it quickly.

[22] As we saw in the rejection of dissection on the grounds that the intervention altered the subject, the human or animal body, which was the object of the investigation.

first the controversiality of some methods; then the major question I began with, of when research was going to do any more than just confirm existing theories; and finally the problem of whether the results would gain acceptance if they *did* challenge common assumptions, particularly difficult, no doubt, when the discoverer was acting solo (like Archimedes or Aristarchus) rather than as a member of a team (like the Mesopotamian *ṭupsarru*).

Sometimes the agenda was set by the state, or was otherwise ideologically loaded, and the more controversial techniques (such as human dissection and vivisection) certainly depended on the support of those in power. The recurrent problem was, of course, how to interpret the data thus collected. Much of it was used simply to bolster pre-existing theories, though the tenacity with which those theories were maintained is, of course, understandable, at least where they had already proved themselves over a certain range of phenomena, as Lakatos (1978: chh. 1 and 2) for one insisted.

However, we do find exceptions, often where new findings surprised the observers themselves, as in the case of the precession of the equinoxes, and the identification of the structures we call nerves as nerves. Yet when heuristics led to the suggestion of challenging new ideas, that was not the end of the matter, for the discoverers had to carry enough of their constituency to get those ideas accepted, and it was far from a foregone conclusion that this would be achieved. The discovery of the nerves gained limited acceptance among elite doctors, but precession encountered resistance in late antiquity despite the fact that the evidence for it increased over time. Meanwhile, to the ancients, heliocentricity seemed to face insurmountable difficulties. If we ask in what spirit or with what aims the ancient investigators worked, they would, no doubt, have said that they were after the truth, but we can see that they were also often out to build up their own reputations, obtain state patronage, and do down their rivals. Finding the truth was one motivation, but competitiveness was often the main driving force, though the circumstances in which rivalries were played out vary from one ancient civilization to another. These are questions that I shall take up in my next study, devoted to the fundamental assumptions the ancients made about what there is to be investigated and why they thought it was worthwhile.

4

Ontologies and Values

Two of the questions I raised at the outset concerned what was assumed about what is there to be investigated and what investigation is good for. If we ask those questions about today's science, the answers are going to be many and various. On the issue of objects, first, sometimes we have little difficulty in identifying the material substances under investigation. Chemistry studies the structure and interactions of chemical elements and compounds, and biology nowadays investigates DNA sequences that are also crucially relevant to medicine and to evolutionary physical anthropology. The status of those objects appears relatively unproblematic,[1] less problematic, at any rate, than those investigated in fundamental particle physics at the frontiers of debate between quantum mechanics and relativity. Some philosophers of science point out that some items are the products of experimentation, not so much discoveries (according to some) as inventions. The further down the road from applied to pure mathematics we go, the more doubts may be expressed as to what, if anything, in the physical world corresponds to the items under discussion. At the limit the subject matter is not objects in any ordinary sense, but equations.

Where much earlier systems of belief are concerned, the temptation is to assume that in their case, at least, the investigators faced no such complications but stayed very much on the surface of the phenomena, whatever they were. The ancients obviously had no clear idea of pure chemical elements, no genes, no quantum mechanics. Yet in other ways they too were confronted with ontological questions. Let me

[1] However, Hasok Chang's (2012) recent discussion of water explores the complex problems thrown up by the history of its analysis. He insists both on the open-endedness of the controversy between opposing views and on what could be said in support of different theories, including those that came to be thought defeated.

identify three examples of this. First, not all ancient investigations presuppose a substance-based physics, for some are oriented towards processes. Second, there are problems to do with the relations between mathematics and physics. Third and relatedly, there are other more general issues to do with the gap between the appearances and an underlying reality. Those questions will take me to the second main issue I shall tackle in this chapter, namely what ancient investigators thought their investigations were good for. For that, we need to examine the values and motives of those investigators and indeed the goals they set themselves. In the modern notion, science, as we understand it, not only provides insight into nature but is the key to sustained material progress. But how far either of those ideas has an exact equivalent in ancient societies is, as we shall see, problematic.

It may once again be objected that what ancient researchers were doing was not science in the first place, and as I remarked earlier, no ancient civilization had a term that exactly corresponds to 'scientist'. Of course, most of their theories and explanations have been superseded. But we should not judge science by its results, for they are always revisable, never definitive, though some are more robust than others, to be sure. The question becomes more interesting and more complex if we ask what the aims and methods of ancient inquiries were. I have had quite a bit to say about methods already, and as for aims, they certainly sought, among other things, better understanding, even though their notions of what counts as proper understanding differed. Some of them focused on causal explanations, others on classification—on appreciating the connections between things—yet others on how to manipulate objects for practical ends, and still others, as we shall see, on what contributes to happiness, when that was seen to depend on 'understanding'. We may choose to be abstemious about the terminology of 'science': that is a policy I endorse in principle, though it involves many circumlocutions in practice. But my twin questions remain: what was assumed about what there is to investigate, and what good was supposed to come from their investigation?

The issue between substance-based ontologies and process-based ones can be dramatically illustrated by an episode in 16th-century history of science, the misunderstandings that arose when the Jesuits arrived in China (Gernet 1985). They still maintained an Aristotelian four-element theory, based on earth, water, air, and fire, and thought their 'physics' far superior to the standard Chinese ontology based on

the *wu xing*, which we nowadays translate as 'five phases', where the basic sense of *xing* is activity, as in walking or travelling—which should certainly have alerted the Jesuits to a possibly different agenda. The five in question are wood, fire, earth, metal, water, which the Jesuits took to be elements. So for them, the issue was simply one between a four-element theory and a five-element one, and the inferiority of the Chinese view could be seen by their inclusion of wood and metal as two of the *xing* and their exclusion of air (which ignored Chinese ideas about *qi*, which means energy, breath, air: but *qi* is not one of the phases since it does not participate in, but rather underlies, the cycles that govern their transformations).

But the *wu xing* are not elements, but processes, linked in two cycles, one of mutual production, one of mutual conquest (Figure 4.1). In the generation cycle, wood produces fire which produces earth which produces metal which produces water which produces wood to start the

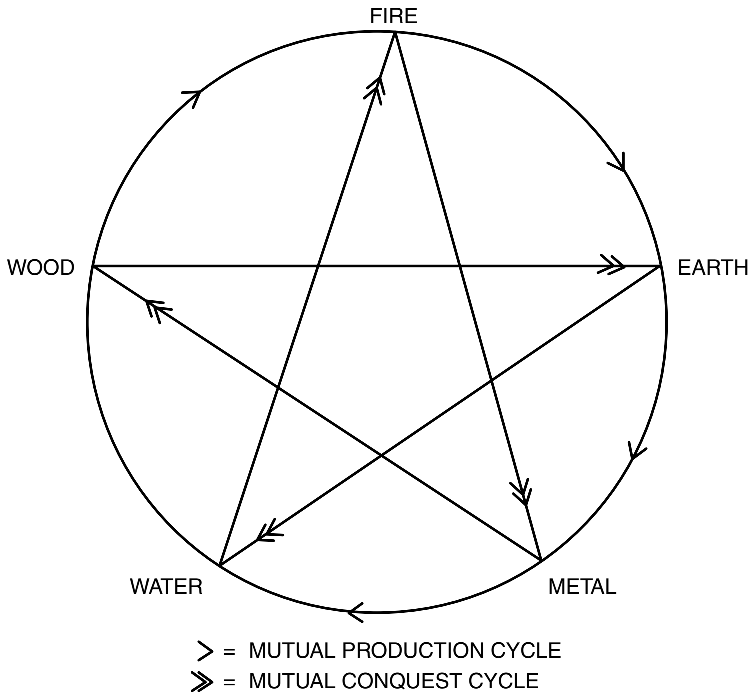


Figure 4.1 The Chinese *wu xing*—five phases. The mutual production and mutual conquest cycles

whole cycle over again. In the mutual conquest cycle, wood overcomes earth, metal overcomes wood, fire overcomes metal, water overcomes fire, earth overcomes water, and wood overcomes earth again to restart the cycle. Some of these productions and conquests make good enough sense at the level of obvious phenomena: water puts out fire and fire melts metal. But we must be careful. The five phases are in constant interaction. But they are not stable substances but processes. One of our earliest cosmological texts, the *Hong Fan* in the *Shang Shu* (Karlgren 1950: 28 and 30), dated to some time between the early 4th and the mid 3rd century BCE, says explicitly: 'Water means (*yue*) soaking downwards, fire means (*yue*) flaming upwards.' Of course, water is also what you drink, but even here the usual Chinese view of the body (as we have seen) is of constant interactions. Here, as elsewhere, the Jesuits assumed that the Chinese had the same agenda, the same conceptual framework, as themselves, a move that greatly facilitated their conclusion as to their own superiority. If they had been better historians, they might have stopped to consider what could be said for an alternative, process-based view of what is fundamental. But of course, they were not historians, but missionaries.

But it is not even as if all Greek theorists agreed with the Aristotelian picture of four elements, each characterized by a pair of the four primary qualities, hot, cold, wet, and dry. For the 5th-century-BCE atomists all that exists is atoms and the void, the atoms differentiated in shape, arrangement, and position alone. Even before them Heraclitus called the *kosmos* an 'everliving fire, kindled in measures and extinguished in measures'.[2] Water, for him, was a paradigmatic example of change (you cannot step into the same river twice),[3] constant change according to the interpretation I favour, a picture that is far closer to the Chinese than was Aristotle. Even in Aristotelian four-element theory, fire posed problems. Aristotle's pupil and colleague Theophrastus argued that fire is quite different from the other three simple bodies, since it requires a substratum (fuel) while they do not. 'Everything that burns is always as it were in a process of coming-to-be, like movement [*kinēsis*]. And so it perishes in a way as

[2] Heraclitus Fr. 30: 'This cosmos, the same for all, no god nor human made, but it was always and is and will be an everliving fire, kindled in measures and extinguished in measures.'

[3] Heraclitus Fr. 91, cf. Fr. 12.

it comes to be, and as soon as what is combustible is lacking, it too itself perishes along with it' (*On Fire* ch. 3, Coutant 1971: 5.8ff).

Here then are good examples of fundamental divergences, first between some Greek theories and some Chinese ones, and then within Greek views, on basic ontological questions, on whether substances or processes are primary, and on whether matter is or is not infinitely divisible, where it is as well to recognize that the issues are well beyond the reach of easily accessible data to resolve. Indeed in some cases, where we are dealing with what I call the multidimensionality of the phenomena,[4] it is more a question of seeing which aspects of those phenomena are captured by either side than attempting to decide between them, in the sense of concluding that one is right and the other wrong, as if the pair of them offered mutually exclusive and exhaustive alternatives.

The controversy between atomism and continuum theory in Greece illustrates another aspect of the debate rather clearly. In the background, as Furley (1987) and Sedley (2007) have shown, was often the issue of teleology, the question of whether the cosmos exhibits benevolent design—less a problem in physics than a matter of a philosophical, indeed moral, attitude. The same well-known phenomena were invoked but explained differently on either side. Some thought that the fact that fish swim in water shows that the void is not necessary to explain movement (as the atomists had maintained). But the atomists countered that there are invisible voids scattered through the water just as they are in every other liquid or solid.[5]

Even when new empirical evidence was brought to bear, this did not settle the matter. Hero of Alexandria in the 1st century CE has an extended discussion to refute those who denied the void and to establish that one can be produced artificially.

> Those who assert generally that there is no void are satisfied with inventing many arguments for this and perhaps seeming rather plausible with their theory in the absence of any perceptible proof. If, however, by referring to the appearances and to what is accessible to perception, it is shown that there is a continuous void, but only one produced contrary to nature; that there is a natural void, but one scattered in tiny quantities;

[4] See Lloyd 2007a and 2012a.

[5] The argument from fish swimming in water is reported and answered, for instance, in Lucretius *On the Nature of Things* I 370–83.

> and that bodies fill up these scattered voids by compression: then those who put forward plausible arguments on these questions [understand: without empirical evidence] will no longer have any loophole. (*Pneumatics* I 16. 16–26)

He goes on to describe setting up his apparatus, a bronze sphere tightly sealed on every side, except for a hole pierced into it at one point, into which a siphon is inserted. This is overly elaborate for the purpose, obviously, but the elaboration is part of the rhetorical display. He shows first that air can be forced into the globe, and then also that it can be drawn out of it, which he claims proves conclusively that a considerable accumulation of void occurs in the sphere. The trouble is, of course, that continuum theorists would deny that the test demonstrates the possibility of a void, natural or artificial. They would simply invoke the assumption of the elasticity of matter to give an alternative interpretation of the results that Hero reported.

I shall have more to say about inconclusive debate later, but now let me turn to my next ontological problem to do with the relationship between mathematics and physics, which I touched on in Chapter 3. In both China and Greece musical harmonies were the subject of intense mathematical investigation, but while the principal concords of octave, fifth and fourth, were everywhere recognized to correspond to ratios of 2:1, 3:2, and 4:3, on other issues there were considerable disagreements. In China these related, for instance, to whether or how far approximations are to be allowed in analysing the twelve-tone scale. In one discussion in the 3rd-century-BCE text, *Huainanzi*, it is shown how the twelve notes of the chromatic scale can be generated in sequence by alternate ascents of an interval of a fifth and descents of a fourth (Figure 4.2), in a way that is reminiscent of Aristoxenus's use of a similar method to get the notes in an octave. But in calculating the numbers to be assigned to each of the notes, the text in *Huainanzi* allows itself two licences. First, the alternation of ascents and descents is interrupted, with two descents in succession (from the bell named 'Responsive Bell' to 'Luxuriant' and then again from it to 'Great Regulator'). Second, it 'rounds' some of the numbers assigned to the notes, once not even taking the nearest whole number.[6]

[6] Thus 'Responsive Bell', the first after the five main pentatonic notes, is assigned the number 42, though by strict arithmetic 64 (the number of the previous note) multiplied by 2/3 (that is the interval of a fifth) gives 42 2/3, which one would have expected to be 'rounded' to 43.

Pitch pipes in generation order	Pentatonic notes	Modern interpretation	Interval	Number assigned In Huainanzi 3
Yellow Bell	*gong*	C	Fifth	81 (×2/3)
Forest Bell	*zhi*	G	Fourth	54 (×4/3)
Great Budding	*shang*	D	Fifth	72 (×2/3)
Southern Regulator	*yu*	A	Fourth	48 (×4/3)
Maiden Purity	*jue*	E	Fifth	64 (×2/3)
Responsive Bell		B	Fourth	42 (×4/3)
Luxuriant		F sharp	Fourth	57 (×4/3)
Great Regulator		C sharp	Fifth	76 (×2/3)
Tranquil Pattern		G sharp	Fourth	51 (×4/3)
Pinched Bell		D sharp	Fifth	68 (×2/3)
Tireless		A sharp	Fourth	45 (×4/3)
Median Regulator		F		60

Figure 4.2 Chinese harmonics: the generation of the chromatic scale in *Huainanzi* 3 21b–22a

But elsewhere in Chinese harmonic theory such roundings are not used—at the price of coping with ratios involving large numbers. In the *Shiji* 25: 1249.2ff., where the sequence of alternate ascents and descents is adhered to strictly, that means dealing with such a ratio as that of 32,768 to 59,049. Indeed in a second discussion in the *Huainanzi* itself (3.21a), when fractions and roundings are not used, numbers up to 3^{11}, that is 177,147, will be needed.

In Greek debates the alternatives often reflected competing epistemologies (Barker 1989 and 2000). Plato already reports a broadly empirical programme of which he strongly disapproves. In the *Republic* 531a Socrates mocks those who try to settle the main questions by appealing to perception.

> Measuring audible concords and sounds against one another, they exert themselves to no avail, just as the astronomers do . . . They talk of things they call minims and laying their ears alongside, as if trying to catch a voice from next door, some

> state that they can hear a note between, and that this is the least interval, to be used as the unit of measurement, while others protest that the strings now render identical sounds.

Both sides, Socrates complains, 'prefer their ears to their minds'. But why not? one might ask.

Plato there has his own particular agenda, a political one indeed, the training of philosopher-kings no less. For their education, to train them in abstract thought, the study of the Forms, and eventually the Form of the Good itself, he advocates a purely rational investigation into which numbers are inherently concordant and which are not, and why in each case. Plato himself does not go into any detail, and there is some dispute in modern scholarship about what that programme might look like.[7] But other epistemological disputes reverberate down to late antiquity, some advocating a quasi-geometrical analysis of the principal concords, others a more purely arithmetical one, with dire consequences on a number of issues.[8]

Thus should one say that the octave, the fifth, and the fourth are exactly six tones, three and a half tones, and two and a half tones respectively? Everything will depend on how a tone is defined. If it is taken as the ratio of 9:8, then you do not get an octave by taking six such intervals, which would give you 9/8 to the power of 6, rather than 2 to 1. According to one view the excess of a fifth over three tones, and of a fourth over two, should be expressed by the ratio 256:243, not by the square root of 9/8—though of course, the distinction between those two ratios is a purely mathematical one and they cannot be discriminated aurally. Again can a tone be divided exactly into two equal intervals, where in the Pythagorean tradition, represented by the *Sectio Canonis* in the Euclidean Corpus, it is shown that there is no ratio of integers that corresponds to a half or a quarter tone? But against those who reduced the whole subject to a branch of number theory, Theophrastus pertinently remarked that what is heard is not a number (nor a ratio between numbers), even if concords are expressible numerically. 'If, however, a quantity belongs to the notes in the same way as it does to colour, being something different [from the quantity itself], then a note is one thing and the quantity related to it another.'[9]

[7] See Mueller 1980, Mourelatos 1980, 1981.

[8] The details of the debate are well set out by Barker 1989 and 2000.

[9] An excerpt of Theophrastus's lost work *On Music* is quoted by Porphyry in his *Commentary on Ptolemy's Harmonics* 61.22ff. (Düring). The text I quote comes at 62.10–12.

My next example will illustrate both the problem of the status of mathematical objects (though I cannot here go into the intriguing problems that opposed Platonic to Aristotelian and other philosophies of mathematics (cf. e.g. Lear 1982)) and the more general issue of appearance and reality. This relates to the Greek use of geometrical models for the movements of the heavenly bodies. It is well known that neither the Mesopotamian nor the Chinese astronomers attempted such representations, at least so far as our extant evidence goes. They just got on with the business of constructing as accurate tables as possible in order to determine periodicities which both knew to be inexact and which the Chinese assumed to be subject to variation. Where many (but not all) Greek theorists assumed that the heavens are unchanging, the Chinese thought on the contrary that they are subject to change and that the calendar, for instance, had continually to be updated (see Martzloff 2009). I say many but not all Greeks, since Plato was one notable exception (though this does not always get taken into consideration in the handbooks), for he clearly stated in the *Republic* (530ab) that as the physical objects they are, the heavenly bodies are liable to change.[10] However, geometrical modelling is an exceptional Greek preoccupation, driven in part by those, including Plato himself,[11] who considered it a scandalous misrepresentation of the facts to suggest that the movements of the planets are irregular (as their Greek name 'wanderers' implied).

So later commentators report that Plato set astronomers the task of 'saving the phenomena', that is reducing the apparent irregularities of the movements of the sun, moon, and planets, to combinations of

[10] [The true astronomer] will consider that the craftsman of heaven fashioned it and all it contains in the best possible way for such things: but when it comes to the proportion of night to day and of these to the month and of the month to the year, and of the other stars to these and to one another, do you not suppose that he will regard as a very strange person anyone who believes that these things go on for ever without change or the slightest deviation, even though they possess bodies and are visible objects?

That leads him to a radical new suggestion about what the study of the heavens is good for, at least in relation to the education of the Guardians: It is by making use of problems, then, as in the study of geometry, that we will pursue astronomy too, and we will leave the things in the heavens alone if we are to participate in the real astronomy and so make the natural intelligence of the soul useful, instead of useless.

[11] At *Laws* 821b–822c Plato has the Athenian Stranger emphatically deny that the planets 'wander'.

regular, circular motions.[12] Whether Plato really did initiate any such programme is doubtful. We have already seen that in the *Republic* he was chiefly interested in astronomy as a training in abstract thought, where the focus was on numbers and figures in themselves, and for that purpose you could ignore the observational data completely ('leave the things in the heavens alone'). Yet in the cosmology in the *Timaeus* he certainly does pay attention to the data available, including the relative speeds of the planets and the ways they overtake, and are overtaken by, one another.[13] Nevertheless it is clear that from the 4th century BCE onwards successive attempts were made to account for those movements, using first combinations of concentric spheres, then epicycles or eccentrics or combinations of them.

But what was their status? Under the influence of Duhem (1908) especially, many modern historians took it that they were just calculating devices that corresponded to no reality. But the chief text in Proclus writing in the 5th century CE that Duhem used to support his view simply does not say what Duhem took it to be saying.[14] Proclus objected to both the realist and the instrumentalist accounts of epicycles and eccentrics, which he knew not to have been in Plato.[15] At *Outlines* 236.15–18 Proclus sets out the alternatives. 'For what are we to say about the eccentrics they go on about and the epicycles? [Are we to say] that they are merely contrivances [that is objects of thought] or that they also have existence in their spheres in which they are fixed?'

Proclus does not like the realist view (*Outlines* 236.25–238.6), since epicycles and eccentrics would destroy the continuity of the spheres: and why should some move in one direction, others in another? In the

[12] In his *Commentary on Aristotle's On the Heavens* 488.19ff., Simplicius cites Sosigenes as evidence for this recommendation of Plato's, and it is possible, though far from certain, that (as Simplicius suggests) Sosigenes himself was drawing on the history of astronomy by Aristotle's associate Eudemus. See also, for example, Simplicius, *Commentary on Aristotle's Physics* 292.18ff. Quite what different Greek writers may have meant by 'saving the phenomena' has been the subject of considerable debate: see for example Duhem 1908; Wasserstein 1962; Wright 1973–4; Smith 1982; Lloyd 1991: ch.11.

[13] Plato, *Timaeus* 38d ff. Whether Plato's talk of the planets 'overtaking' and 'being overtaken by' one another implies that he was aware of retrogradation is again controversial: see Bowen 2001, 2002.

[14] I undertook a detailed examination of the main text in Proclus that Duhem cited, the *Outlines of Planetary Hypotheses* (hereafter *Outlines*), and of his translations of key passages in Lloyd 1991: ch. 11.

[15] This is clear from Proclus's *Commentary on Plato's Timaeus* III 56.31ff., 76.28ff., 96.19ff., and from his *Commentary on Plato's Republic* II 214.6ff., 227.23ff.

latter case his point may be that the moon and sun move on their epicycles in a sense opposite to that of the epicycles on their deferents.[16] But he objected to the instrumentalist view as well. If they are just contrivances, he says (*Outlines* 236.18–22), 'one has unwittingly gone over from physical bodies to mathematical concepts and given the causes of physical movements from things that do not exist in nature'. Moreover (236.22–25), it is absurd to put mere objects of thought in motion and to have them cause motion.

Proclus is in some difficulty about the detail of the movements of the heavenly bodies, though he sticks to realist assumptions on the position of the earth and on there being heavenly spheres.[17] But then Proclus was no practising astronomer. The chief astronomical theorist for whom we have ample evidence from his own extant texts, Ptolemy, in the 2nd century CE, undoubtedly both uses epicycles and eccentrics and opts for a realist interpretation of them. In his work on the *Planetary Hypotheses* he gives an account of the actual arrangements of physical objects in the heavens: Book 2 chapter 6, 117.35ff. (Sambursky 1962: 142) speaks of the tambourines or segments of spheres on which the planets are carried. 'It is not proper to suppose that there are in nature superfluous things which do not make sense, namely complete spheres for motions for which a small part of these spheres would suffice [namely segments produced by two parallel cuts on both sides of the circle along which the planet is carried around in its epicycle].'

Moreover, he offers a vitalist account of how the celestial bodies move (Book 2, ch. 7, 119.21ff., 120.6ff., Sambursky 1962: 142–4): 'We have to suppose that among the celestial bodies each planet possesses for itself a vital force and moves itself and imparts motion to the bodies united with it by nature.'

On the one hand it is absolutely correct to say that some Greek theorists, such as Autolycus of Pitane in the 3rd century BCE, wrote treatises (his was called *On the Moving Spheres*) dealing purely with

[16] Again 'they confound their relative distances, if sometimes they [the circles] are brought together in a single plane, but sometimes are separated and cut each other. Thus there will be all kinds of divisions and foldings-up and separations of the heavenly bodies' (*Outlines* 238.3–8).

[17] See Proclus *Outlines* 28.7ff. and 28.21–5, and *Commentary on Plato's Timaeus* III 137.6ff. on the position of the earth, and *Commentary on Plato's Timaeus* III 128.14ff. on the substance of the heavenly spheres.

the solutions to the mathematical problems that are needed for astronomical theory. We can certainly say that the role of the mathematician was distinguished from that of the physicist, though the former, we are told by Geminus, quoted by Simplicius (*Commentary on Aristotle's Physics* 292.22ff.) depends importantly on the latter.

> It is not the business of the astronomer to know which bodies naturally rest and which move, but he introduces hypotheses . . . and considers from what hypotheses the appearances in the heavens will follow. But he must take his starting points/principles from the physicist, namely that the movements of the stars are simple and regular and orderly.

It is also true that for several important Greek astronomers, we do not have good concrete evidence to establish just what they believed or assumed on the question of the physical correlate to their mathematical models. Yet on the other hand, wherever we do have such evidence (notably in Ptolemy himself, but also in Hipparchus),[18] that suggests that their aim was not simply to give a mathematical account but one that is true of the real world—not just an instrumentalist theory, in other words, but a real physical one, difficult as that proved to be. More was at stake than just mathematical solutions to which the observational data could be fitted. The underlying motivation was to show that the cosmos is well ordered.

So we come now to my second main question, of what 'science' was good for in the ancient world. The question is all the more poignant since, as I have said before, the ancients often admitted they were at a loss to give well-grounded solutions to the problems they tackled, whether they be the constituents of physical bodies, the origin of the world, the causes of diseases, or the detailed models of the planets, as I have just been discussing. The notion that science is valuable since it holds the key to sustained material progress was evidently less important when ideas of such progress were rather limited. But we may break down the possible motives for 'scientific' investigation in ancient civilizations into a number of different categories. First,

[18] At least we are told by Theon of Smyrna (2nd century CE) that Hipparchus adopted the epicyclic hypothesis, rather than eccentrics, not for instrumental reasons but for cosmological ones. 'It is more plausible that all the heavenly bodies should lie symmetrically with regard to the centre of the universe [the earth] and be joined together similarly' (*Exposition of the Mathematics Useful for the Study of Plato* 188.15–19, Hiller). It is noteworthy that Theon himself contrasts Babylonian and Egyptian astronomy with Greek on just this point: the former lack a physical account (*phusiologia*) that the Greeks supplied (*Exposition* 177.18–22).

reputation and jobs (I mean for the investigators themselves); second, practical utility; third, the idea that understanding is essential for happiness; and fourth, the idea that it is essential for good government (the third is especially prominent in some Greek writers, the fourth in some Chinese ones).

First, as we have seen when reviewing the rivalry between competing experts in Mesopotamia, China, Greece, and India, one clear-cut motivation was prestige. As I mentioned in Chapter 2, the victor in the debates in the *Upaniṣads* humiliated his rivals, who were often reduced to being his servants. In both Mesopotamia and China especially, victory in competition with others could well lead to employment, in the courts of kings or rulers. I mentioned the different kinds of experts that received the king's patronage in Babylonia. In China successful graduation from the Imperial Academy founded by Han Wu Di in the 2nd century BCE usually led to a position in the imperial civil service. That academy was founded to train such civil servants. The gurus of the *Upaniṣads* also were recipients of patronage.

In Greece, however, there was no such clear career route. True, the courts of tyrants, kings, eventually the Roman emperors, offered patronage for a variety of intellectuals, including poets, philosophers, historians, and mathematicians. But the Greek academies, from Plato's onwards, were not training grounds for bureaucrats. The instruction they offered in rhetoric, that is skills in public speaking, was undeniably useful for a budding statesman or politician. But success as a politician depended not on your patron so much as on your own ability. So while 'science' as a source of reputation is a theme common to all those ancient civilizations, 'science' as a pathway to employment is unevenly distributed between them.

That takes me then to the question of usefulness, where parts of the answer concerning ancient notions of that are predictable, but some also rather surprising. In both China and Greece the knowledge the scientist obtained from his researches could be put to use in many domains, though most of the innovations in such areas as agriculture, textiles, and military technology were the work of anonymous individuals, who probably had no interest whatsoever in the abstract analysis of their achievements or why they worked. But some weapons of war, especially, were developed by way of the application of the results of deliberate research, in ways that have instantly recognizable parallels in the modern world. A report in Philo of Byzantium

(*c.*200 BCE) gives a circumstantial account of how improvements in catapults were the result of investigations sponsored by the Ptolemies (*On Artillery Construction* ch. 3, 50.20ff., Diels-Schramm):

> Now some of the ancients discovered that the diameter of the bore [the circle that receives the twisted skeins] was the basic element, principle and unit of measurement in the construction of artillery. But it was necessary to determine this diameter not accidentally nor haphazardly, but by an established method that can produce the right proportions for any magnitude, and this could not be done other than by increasing and decreasing the circumference of the bore and testing the result. And the ancients, as I say, did not bring these investigations to a successful conclusion, nor did they determine the magnitude, since their trials were not conducted on the basis of many different types of performance, but merely in a search for the optimum result. But those who came later, noting the errors of their predecessors and reviewing the results of subsequent experiments, reduced the principle and method of construction to a standard element, namely the diameter of the circle that receives the spring. Success in this work was first achieved by the Alexandrian craftsmen, who received considerable support from kings [that is the Ptolemies] who were eager for fame and well-disposed to the arts and crafts. For it is not possible to grasp everything by reason and the methods of mechanics, for many things have to be discovered by trial.

Other examples of useful devices invented by or at least associated with named individuals include, from China, Zhang Heng's seismoscope (Figure 4.3), and from Greece, Archimedes's screw (Figure 4.4), and Ctesibius's fire engine (Figure 4.5). Yet the haul of mechanical inventions that owed something to programmes of deliberate mathematical or technological research in either Greece or China is modest. One of the most brilliant is the instrument found more than a hundred years ago in a shipwreck off the Greek island of Antikythera and so named after it (Figure 4.6). This is a luni-solar calendrical computer incorporating the only extant ancient example of a system of gearwheels, one of considerable complexity indeed. Although the instrument can be dated between 150 and 100 BCE, we have no idea who was responsible for its manufacture.

In medicine, another area where we are used to the effective application of pure research in improved treatments, again the record of

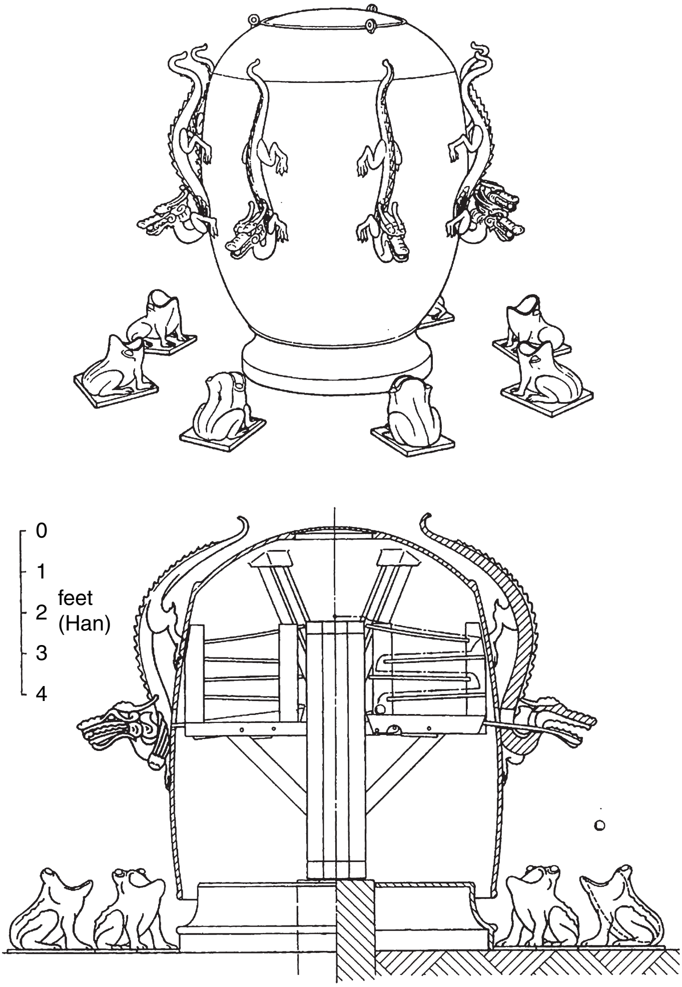


Figure 4.3 Zhang Heng's seismoscope as reconstructed by Sleeswyk and Sivin (1983) from the account in the *Hou Han Shu* (59: 1909)

each of our ancient civilizations is moderate. There are abundant archaeological finds of medical instruments from both China and Greece (see for example Künzl 1983), and these certainly show considerable ingenuity in devising tools for surgical procedures, including dentistry, obstetrics, and trepanation (a technique that goes back long before the classical period: Lisowski 1967; Margetts 1967). We know of Greek innovations in the techniques used for reducing fractures and

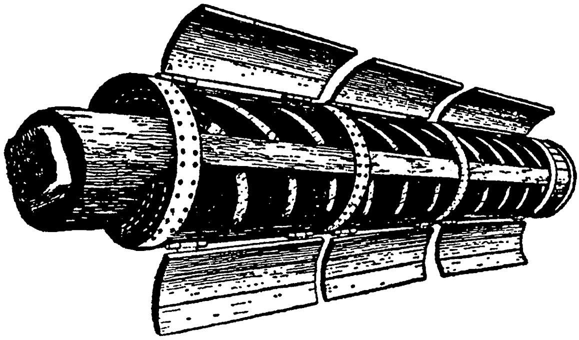


Figure 4.4 Archimedean screw used as a water-lifting device, from Sotiel, Spain

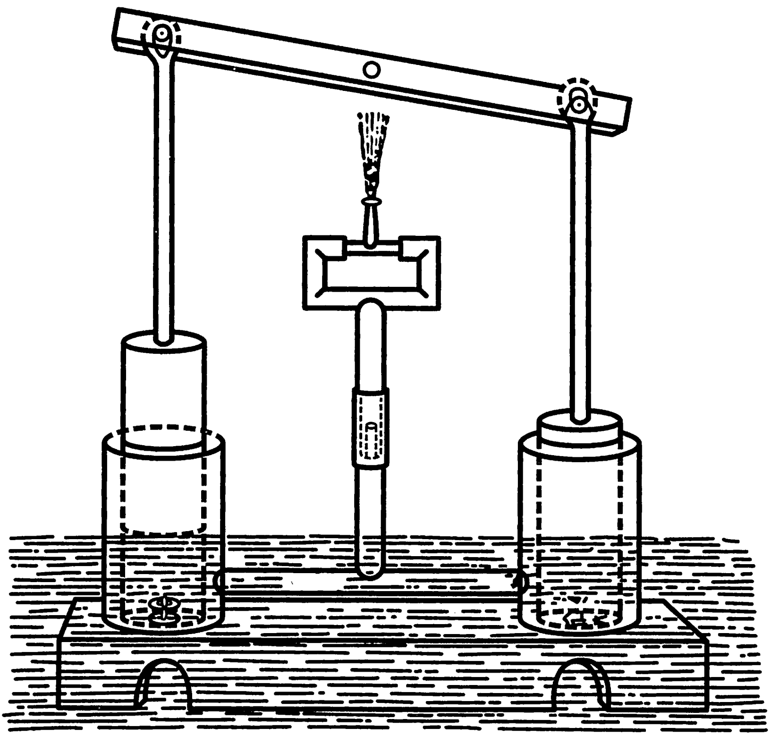


Figure 4.5 Ctesibius's fire engine (3rd century BCE)

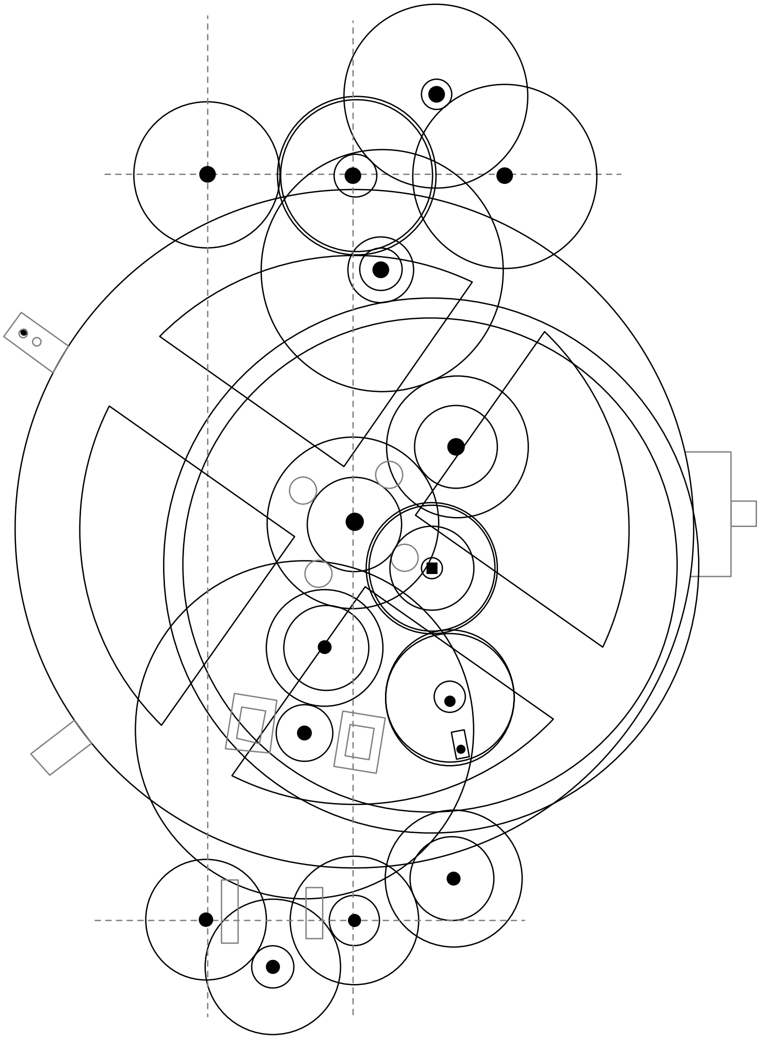


Figure 4.6 The Antikythera mechanism: a reconstruction

dislocations, many making use of quite fancy machinery, and many involving pretty drastic interventions that the more cautious Greek medical writers themselves warned could do more harm than good. The Hippocratic bench bears a strong resemblance to an instrument of torture, the rack (Figure 4.7).

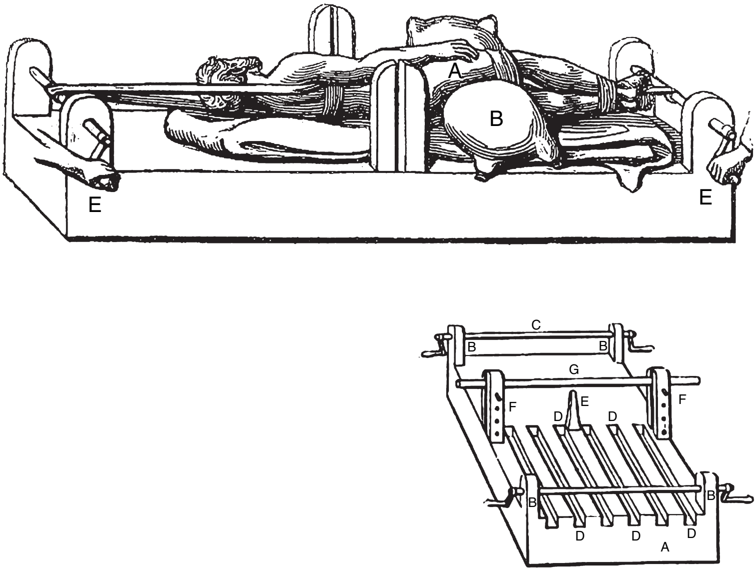


Figure 4.7 Hippocratic bench for reducing dislocations

There is no shortage of *claims* to the effect that the knowledge won is useful, not least among the Greeks, despite their reputation (in some quarters) as being slow to think of utility. But when they did develop that theme, they clearly treated it very broadly. A text from the early 4th-century-CE mathematician Pappus of Alexandria (*Mathematical Collection* book 8, chh. 1–2, III 1022.3ff., Hultsch) is worth quoting in extenso.

He begins with a somewhat optimistic statement of how highly 'mechanics' is regarded: 'The study of mechanics . . . being useful for many important things in life, is with reason thought by philosophers to be worthy of the highest approval and is eagerly pursued by all those interested in mathematics.' He goes on to quote the distinction that Hero of Alexandria had made between the theoretical and the practical sides of mechanics to make the point that the best master craftsman is both a good theorist and a good practitioner.

But then discussing 'the most necessary of the mechanical arts from the point of view of the needs of life', he offers a fivefold classification (1024.12–1026.4).

> (1) The art of the constructors of pulleys . . . with these machines they use a lesser force to raise high great weights against their natural tendency. (2) The art of the makers of instruments

> necessary for war . . . Missiles of stone, iron and the like are hurled great distances by the catapults that they make. (3) The art of the machine makers properly so called. Water is easily raised from a great depth by the water-lifting machines that they construct. (4) The ancients also called the wonder workers mechanicians. Some invented pneumatic devices, as Hero in the *Pneumatics*, others seem to imitate the movements of living things by means of sinews and ropes, as Hero does in his *Automata* and *On Balances*, and others use objects floating on water, as Archimedes in his work *On Floating Bodies* or water clocks, as Hero in his work *On Water Clocks* . . . (5) They also call mechanicians those who are skilled in sphere making, who make a model of the heavens by means of the uniform circular motion of water.

That last example implies some kind of orrery driven by water power, which might suggest something like the Antikythera instrument with the addition of a mechanism powered by a constant flow of water analogous to that in Ctesibius's water clock, though that is pure speculation. In China, too, as I noted before (Chapter 3, p. 78), Zhang Heng is also credited with an armillary driven by water, though again we are at a loss as to how exactly that worked.

It can be seen what a strange mixture this text of Pappus presents us with. The way in which weapons of war are 'most necessary from the point of view of the needs of life' is obvious enough, and so too his first example of weight-lifting devices used in architecture (Figure 4.8). But it is striking that Pappus should include Archimedes's work *On Floating Bodies* when the work that is extant with that title is a purely abstract mathematical discussion. Again when Pappus includes the toys in Hero's work on *Automata* that 'imitate the movements of living things', that is hardly a top priority for ordinary folk in their efforts to meet the needs of life. The 'usefulness' here is rather a matter of entertainment, as was also the case with some of the devices, often just toys or small-scale models, that were designed to produce surprising effects in the service of religion (Figures 4.9 and 4.10). Similar gadgets, some practical, some designed rather for entertainment, can be paralleled in China.[19]

[19] See Needham 1965: 156ff. Ma Jun, in the 3rd century CE, in particular was credited with a number of inventions, including water-lifting devices and ballistae as well as a puppet theatre for court entertainments. He certainly made, if indeed he did not invent, the 'south-pointing chariot' a device mounted on a carriage that always points to the south in whatever direction the carriage itself was moving. This seems to have depended on a system of gearwheels that could be engaged or disengaged to compensate for changes in direction of the carriage itself (Zhang Fan 2006: 496, referring to *Song Shu* 18: 496) (Figure 4.11).

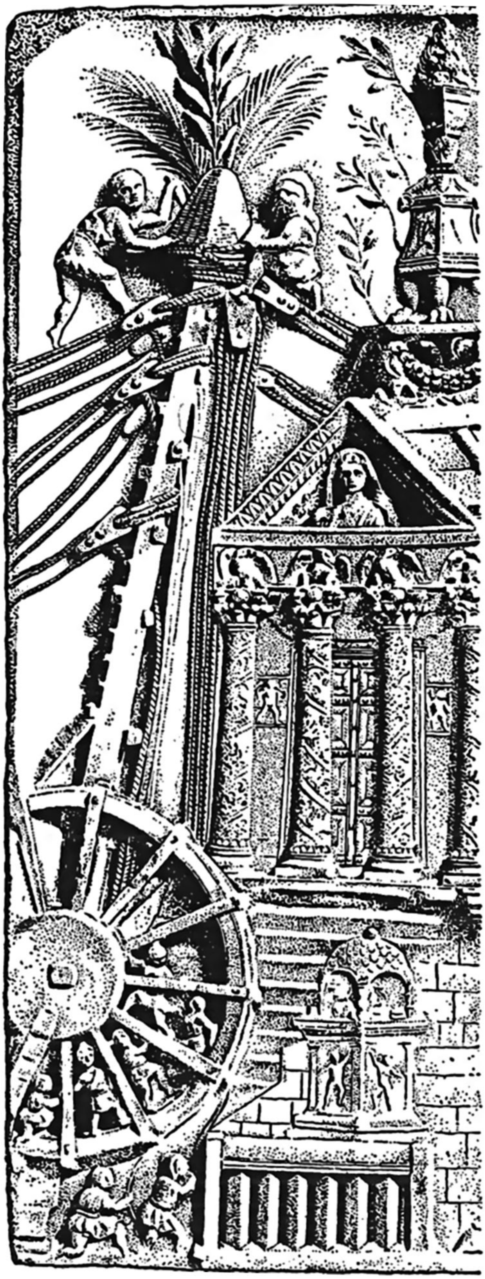


Figure 4.8 Crane with compound pulley worked by a treadmill

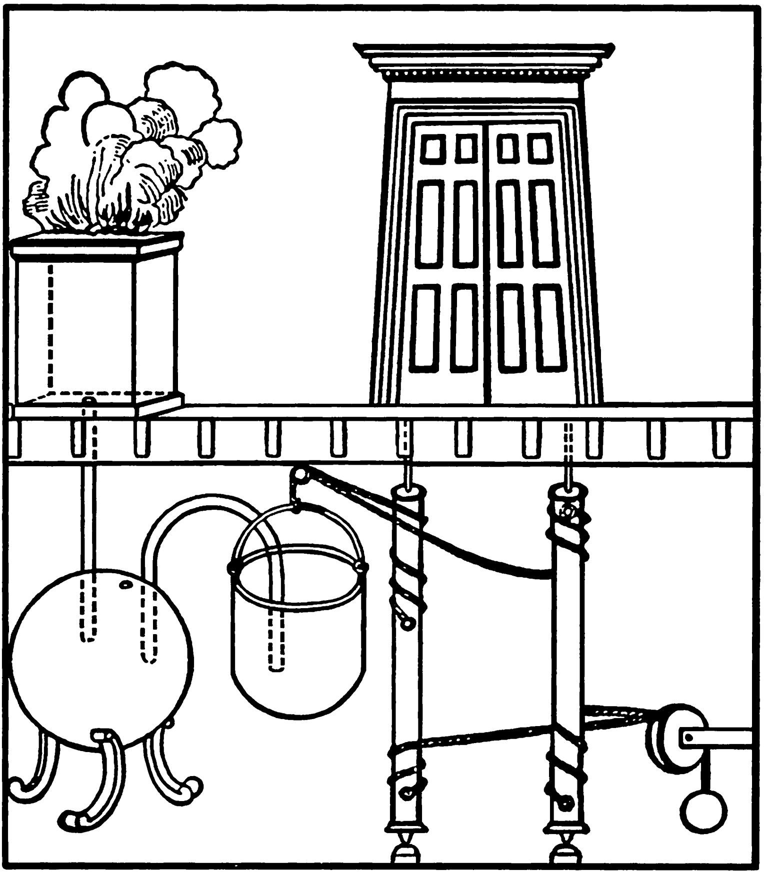


Figure 4.9 Hero's scheme for opening temple doors automatically by the condensation of steam from lighting a fire at an altar

Pappus is hardly a typical Greek author in his period, but his account clearly invites us to explore a very different set of values from those we are used to, and this takes me to the heart of the answers to the question of what 'science' was good for in ancient civilizations. I have a little more to say about the Greeks before I turn back to China.

Many of the main Greek natural philosophers had a very different notion of the usefulness of their investigations from that in Pappus. For Aristotle in particular the *understanding* achieved was a key ingredient in what counted for him as happiness, *eudaimonia*, better

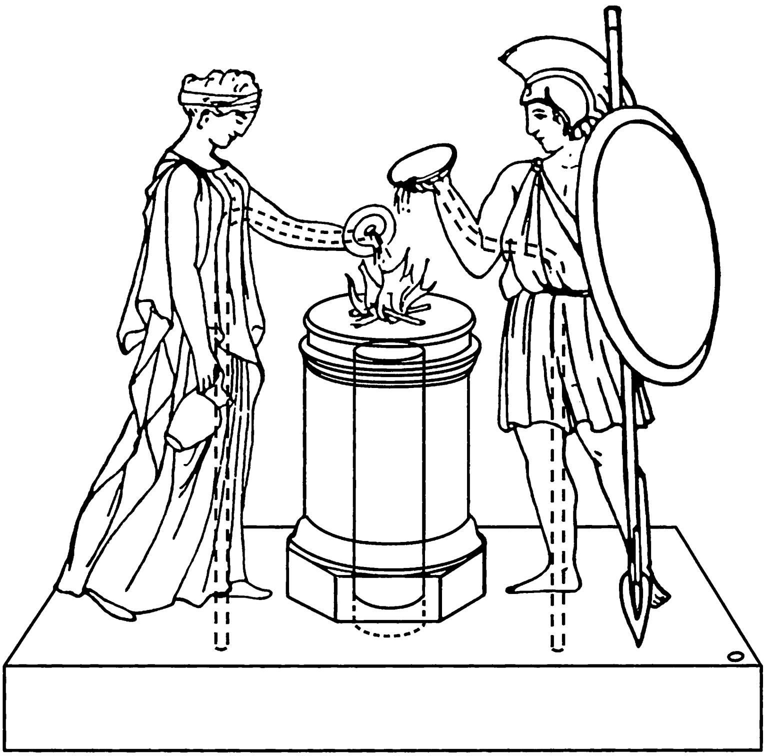


Figure 4.10 Libations at an altar produced by fire

'blessedness', which he defined primarily in terms of intellectual activity. When the Hellenistic philosophers took up the theme of what philosophy could do for you, the recurrent response was to secure peace of mind, *ataraxia*, a key ingredient for them in their notions of happiness, even though Stoics and Epicureans otherwise disagreed on what that consisted in, the Stoics identifying virtue as the supreme good, the Epicureans pleasure.

But both schools argued that 'physics' in their sense, that is the study of nature, is essential for happiness, though they differed on why and how this is so. The Stoics studied natural phenomena the better to appreciate that the world is the product of divine reason which governs it down to the last detail. But the Epicureans studied the same phenomena in order to rid the mind of superstitious beliefs, the traditional notion that the gods intervened, causing earthquakes,

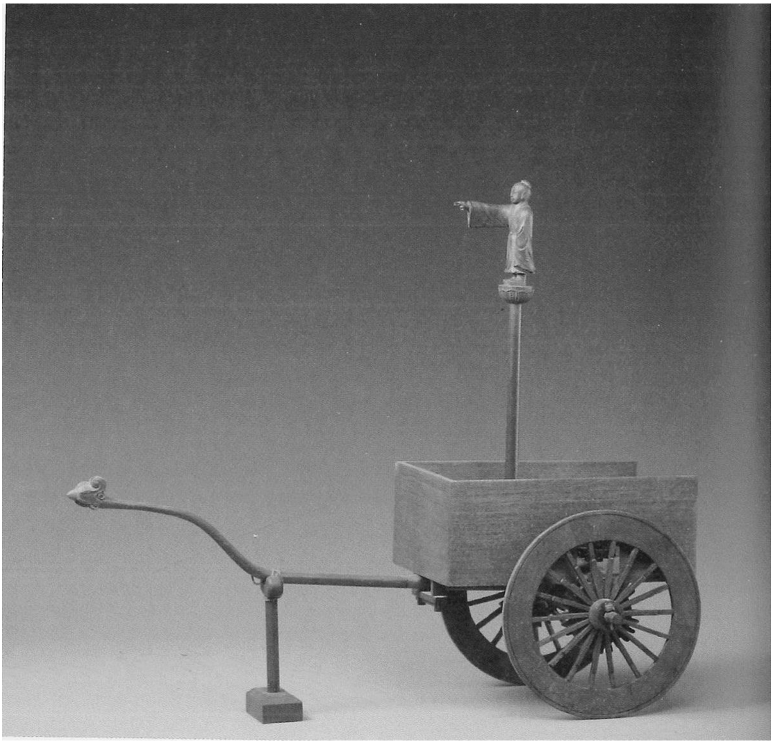


Figure 4.11 Ma Jun's south-pointing chariot

eclipses, and the like. Two texts in Epicurus indicate his attitude. In his *Principal Doctrines* 11 he says: 'If we were not troubled at all by apprehensions about phenomena in the sky and concerning death, that they might mean something to us, and again by our failure to perceive the limits of pains and desires, we should have no need of the study of nature.' And in his *Letter to Pythocles* 85 he rams home the point: 'Bear in mind that there is no other end to the knowledge of things in the sky . . . than peace of mind and firm confidence.'

It is striking that even the Sceptics, who argued that on matters to do with hidden reality and invisible causes the correct stance was to withhold judgement, also held up *ataraxia* as a goal, but *ataraxia* that did not come from solving the problems of research but from seeing that they are insoluble. Yet this required hard work and dedication.

Their ideal was a life devoted to inquiry (from which they got their name Sceptics: literally, Inquirers). But if they wrestled with the problems, that was not in the expectation of resolving them, but rather to appreciate that no solution is to be had. Sextus Empiricus says:

> The Sceptic has the same experience as they say happened to the painter Apelles. Once, they say, when he was painting a horse and wanted to represent in the painting the horse's foam, he was so unsuccessful that he gave up and flung at the painting the sponge on which he used to wipe the paints off his brush. And the mark of the sponge produced the effect of a horse's foam. So too the Sceptics hoped to attain peace of mind by arriving at a decision concerning the disagreements between the phenomena and the objects of thought, and being unable to do that, they suspended judgement. And they found that peace of mind, as if by chance, followed upon that suspense, as a shadow follows its substance. (*Outlines of Pyrrhonism* I, 28–9)

Remarkably despite the fact that time and again natural scientific inquiry in Greek antiquity reached an impasse, it was still valued by those who engaged in it as crucial not just for the ever-elusive understanding that it could yield, but for happiness itself. We even find some adding that it had an important contribution to make to ethics. In his *Syntaxis* I 1, Heiberg I 7.17–24, Ptolemy justifies the study of astronomy in those terms:

> Of all studies this one especially would prepare men to be perceptive of nobility both of action and of character: when the sameness, good order, proportion and freedom from arrogance of divine things [he means the stars, of course] are being contemplated, this study makes those who follow it lovers of this divine beauty and instils, and as it were makes natural, the same conditions in their soul.

If you study the good order of the stars, you become more orderly yourself. A nice idea: if only it were true.

The overwhelmingly egocentric character of Greek justifications for research (tempered, only occasionally, by some of those other modes of utility that we found in Pappus) stands in sharp contrast to the values that predominate in China. What the Chinese legendary Sage Kings stood for—who provided models that members of the intellectual elite should try to embody—was very different. Those kings were paragons of dedication to the welfare of their people, to 'all under heaven' (*tian xia*) as it was said—that means China, of course. To be sure, the way they achieved

this goal differed. Some (such as King Yu) were very definitely hands on: in fact, Yu wore himself out in his efforts to stem the flood, encourage agriculture, and so on. But sometimes the ideal was that of 'doing nothing', *wu wei*, where the Sage Ruler delegated everything to his subordinates—who accordingly had to be chosen with particular care. The inactive Sage Ruler showed his Sagacity not so much in the knowledge he displayed, but supremely by his grasp of the *Dao*, a matter of embodying it, *not* being able to give an account of it, of *logon didonai* as the Greeks would have said. But their success was demonstrated not just by their own personae, but by the prosperity of their people.

Chinese philosophers, as we have seen before, were sometimes called 'itinerant persuaders', targeting the ruler, the emperor himself after the unification. But there is a strong tradition in China that the duty of the persuader is to deter the ruler from actions that were to the detriment of his people and to get him to focus on what would ensure their prosperity. The primary underlying motivation was to advise on good government, government which would ensure the welfare of all.

This overriding concern provides the background for much of the detailed work that Chinese investigators did in the several domains of inquiry. As I have noticed before, the study of the heavens was essential, among other things, so that the ruler could keep the calendar in order (essential for good harvests, after all). The great Chinese universal histories include treatises on waterways, on irrigation, and the like, evidently geared to providing the ruler with essential practical knowledge for agriculture, just as treatises on music were important for the correct performance of ritual.

Of course, the Chinese had their recluses, who practised inaction in a very different environment from the court, in isolation in the mountains. As I noted before,[20] there are some delicious passages in the *Zhuangzi* compilation (4th to 2nd century BCE) satirizing the pomposity of Confucius's courtly rituals. But if there are always exceptions, we have to recall that the education of the Chinese elite steered them towards public office, and the ideal that is clearly expressed (even though many, of course, did not live up to it) was to keep the benefit of all under heaven constantly in mind. In practice many Chinese persuaders remonstrate with rulers whom they see

[20] See above, Chapter 1, p. 26.

going wrong[21]—and many paid for it with their lives, or if not their lives, at least their manhood.[22]

What Chinese inquiry was good for was not just technological progress, but furthering the cause of the general welfare at considerable cost to many of the advisers who had access to those who were in a position to secure it. While Greek inquiries often focused on the happiness of the individual, many Chinese ones take the prosperity of 'all under heaven' as their goal.

The divergence in the dominant values I have identified helps in another way, perhaps, to account for the varying fortunes of investigation in late ancient Greece and ancient China. At the end of pagan Greek antiquity, as at the end of the Han dynasty in China, intellectual inquiry had to compete with a new religious alternative, the rise of Christianity in the West and that of Buddhism in the East. Both religions suggested a very different picture of the world and of our place in it from what had hitherto usually been accepted. For the Buddhists this world is essentially an illusion: the goal is to escape from its entanglements. For Christians salvation became a matter of faith. In his *On Prescriptions against Heretics* (ch. 7), Tertullian famously proclaimed: 'What then has Athens to do with Jerusalem, the Academy with the Church, the heretic with the Christian? . . . We have no need of curiosity after Jesus Christ, nor of research after the gospel. When we believe, we desire to believe nothing more. For we believe this first, that there is nothing else that we should believe.'

There were, to be sure, some less extreme Christian voices who were for making a limited use of pagan natural philosophy. But in general the goal had shifted decisively, away from the intellectual

[21] Lloyd 2005 sets out some of the texts from the Warring States and Qin-Han periods that show the importance of the topic of remonstration for early Chinese thinkers. Cf. Schaberg 1997.

[22] The list of early Chinese advisers who ran into difficulties because of their independent-mindedness is a long one. It includes Lü Buwei, who was prime minister to the man who eventually became the First Emperor, Qin Shi Huang Di, and the compiler of the *Lüshi chunqiu*, who was forced to commit suicide in 235 BCE. Shortly afterwards, in 233 BCE, Han Fei, author of the *Hanfeizi* which I quoted in Chapter 1, was also forced to commit suicide. Then Liu An, the king of Huainan and the compiler of the second great cosmological treatise, the *Huainanzi*, fell out with his nephew, the Emperor Han Wu Di, and he too had to take his own life in 139 BCE. As for Sima Qian, the co-author of the first Chinese universal history (*Shiji*), he too fell out with Han Wu Di and accepted the humiliation of castration rather than committing suicide in order to continue his father's work. Finally, Ban Gu, author of the second great history, the *Hanshu*, was executed in 92 CE.

justification in terms of understanding, of happiness and *ataraxia*, to a very different notion of salvation, where what counted was your eventual fate in the afterlife, bliss in paradise, or eternal damnation in hell. Faced with that alternative, the pagan natural philosophers were no match for Christian proselytism. Reason and inquiry were not the way ahead: faith was.

But in China—despite the radical alternative that Buddhism offered, the justification for inquiry in terms of the welfare of all under heaven and the sense that what research could do was supply the emperor with useful advice—those ideas largely survived in all the turmoil of recurrent dynastic upheavals. The Chinese did not cultivate face-to-face, indeed in-your-face, dispute: and their engagement with epistemology was small compared with the outpourings of the ancient Greeks. But the Greeks paid for that by the perceived impasse that those disputes generated, on physical questions as well as philosophical ones, the nature of happiness indeed. There were fluctuations in the fortunes of research in China too: but (like its political system) it showed greater durability than the Greek counterpart, which owed its survival, of course, to the Arabs, before being transmitted back to Europe to be at the centre of the controversy between the 'ancients' and the 'moderns', between those who saw the Greeks as an inspiration, and those who saw them rather as part of the problem.

I shall return to the major issues of the commonalities and the specificities of the manifestations of human reasoning in my next and final chapter. But let me here draw together the threads of my inquiry thus far into ancient civilizations, while acknowledging that my study has inevitably been highly selective. My discussion has centred on the three interrelated questions of the ideas entertained in ancient civilizations on the right way to go about inquiry, their views on its subject matter and on its value or what it is good for. In each case the answers were in no sense a foregone conclusion. The leitmotif of my analysis has been that there was no single ancient royal road to 'science', but rather multiple routes to different goals, not that by that I mean to imply that *any* goal was acceptable in antiquity, for that was no more true then than it is today. Alongside all the other constraints, social, political, institutional, ideological, under which any investigator worked, there were some that came from the side of the very phenomena they were investigating. An eclipse would not occur just because some ancient *ṭupšarru* said it would. What counted as 'health'

was interpreted differently by different healers: but on any account death was death, and that was true even for those who saw death as a gateway to immortality. In medicine as in other fields of inquiry, the investigator might be more or less successful in persuading others that he knew what he was doing: but those others sometimes had access to more than just his say-so to evaluate his performance, a point I shall come back to in my final chapter.

We must do justice to how different ideals were forged in different circumstances by different ancient investigators. At a very abstract level we may say that all ancient researchers sought a reputation for understanding, but one of my points has been to insist that how understanding was understood differed. For some that was a matter of causal explanations. But for others it was a question of appreciating the associations between things, the kinds or groups they form. We may tend to be dismissive of merely symbolic links, but we have to recognize that the *kinds* of things are a legitimate, even an inevitable, goal of any classification or taxonomy. Nor was there any uniformity in the basic ontological assumptions in play, about what there is to investigate, whether that was substances or processes, things or events, classes or individuals, regularities or anomalies. It is easy to say that astronomers/astrologers studied the heavenly bodies, doctors health and the human body, music theorists harmonies, and so on. But that masks the different preconceptions they brought to the task. My main theme has been the efforts that had to be, and were, expended on the creation of new ideals for inquiry, on what counted as conclusive proofs, on the proper conduct of debate and the need for transparency and accountability there, on devising new techniques of empirical research or new ways of tackling recalcitrant problems.

Of course, ideals are one thing, delivery on them quite another. Each of these ideals faced hazards. Sometimes new methods received the support or at least the acquiescence of the authorities. But sometimes they were developed in the face of considerable opposition, institutional or individual, epistemological or ethical. Observation sometimes reflected what the observers expected or wished to see: experiments quite often begged the question between alternative views; idealizations sometimes helped themselves to unjustified assumptions. Whether or not there were analogues in the Pleistocene, the problems have certainly not gone away. Rivalries could be a major incentive to innovation and to criticism, but in a highly competitive

situation, such as existed in Mesopotamia, India, and China as well as in Greece, they often inhibited it, as when Aristotle observed (*On the Heavens* 294b6ff.) that 'we are all in the habit of relating our inquiry not to the subject matter but to our opponent in argument'.

As we saw, or at least I claimed in Chapter 1, the distinctive Greek ambition to produce demonstrations securing incontrovertible conclusions was a splendid ideal, but the goal it set was often beyond reach. In all the civilizations I have discussed, who could claim to be experts and on what grounds were recurrent issues, and the line between expertise and mere bluff, between robust results and sheer luck, was always hard to draw. In all of them criticism was brought to bear to challenge traditional assumptions and beliefs, but that too could degenerate into radical scepticism, not to say despair, in all of them, and particularly perhaps among the epistemologically sensitized Greeks. I shall accordingly address what we can learn from those common ambitions and divergent manifestations in my final chapter, which sets itself the ambitious task of surveying the broad sweep of the development of human reasoning more generally.

5

Some Great Divide?

THE preceding studies have focused primarily on literate ancient civilizations, where I posed three interrelated questions concerning how various members of those societies thought one should go about investigating, what they believed was there to be investigated, and why they considered the investigation worthwhile. In this final chapter I seek to broaden the scope of the discussion to tackle problems to do with human reasoning and inquiry in general.

Should we take it that human reasoning, the faculty, exhibits certain stable and recurrent characteristics across all human populations across time and space, despite the apparent differences in how that reasoning manifests itself? Or should we admit major differences not just in the end results of reasoning but in the faculty itself, not just in performance in other words, but also in competence or capacity? In either case, how should we account for the differences we can identify? Do they stem from social, cultural, even linguistic, factors, or even basic biological ones?

Let me start with some brief comments on the biological and the linguistic hypotheses. The first type of argument has, of course, been much abused by those who have sought to claim inherent superiority for some supposed Master Race, but it is not limited to their racist rantings, on which I shall not waste any time. Much more soberly, Baron-Cohen (2003), for instance, has provided experimental evidence to distinguish between male and female modes of reasoning, while he acknowledges that not all male humans exhibit the former characteristics, nor all females the latter. Again following up Jaynes's (1976) work on the bicameral mind, McGilchrist (2009) assigns different roles to the right and left hemispheres of the brain, where the balance between their relative importance may vary between individuals, though all for sure possess both hemispheres. There is no doubt

that neurophysiology sheds light on a wide range of cognitive functions (Changeux 1985), though some currently appear to remain well beyond its reach, not least because the correlations between fMRI (functional magnetic imaging) scans and subjective experiences are often problematic (Lloyd 2007a: 60–1). But since I am in no position myself to adjudicate on the experimental evidence, I must limit my own discussion to those aspects of the nature of reasoning on which my comparative studies can, I believe, throw some light.

Then as regards the linguistic hypothesis, here too extravagant suggestions about the suitability of different natural languages to express different types of concepts have been entertained. Bloom (1981), for instance, claimed that counterfactuals not only do not occur in classical Chinese, but that they cannot be expressed in that language. Yet that flies in the face of the facts. It is true that there is no real equivalent, in Chinese, to the manner in which a highly inflected language such as ancient Greek distinguishes different types of conditionals by means of a combination of particular particles, moods, and tenses. Every student of ancient Greek learns the differences between (1) a conditional introduced with the particle *ei* (if) with the verb in both the protasis and apodosis in the indicative, where the apodosis may or may not have *an*, where not having an *an* expresses a fulfilled condition, while having one points to an unfulfilled one, (2) one introduced with *ean* with the verb in the protasis in the subjunctive (expressing a distinct future condition) and (3) those introduced with *ei* but where the verbs in both protasis and apodosis are in the optative (where the future condition is remote).

Although the Chinese language has undergone as many changes as any European one, probably more so, there has never been any problem in expressing basic 'if . . . then . . . ' compound sentences. Thus in the classical period the 'then' clause was regularly introduced by the particle *ze*. Moreover, the incapacity of the Chinese language at any stage in its complex development to express counterfactuals is straightforwardly refuted by the fact that one particle used to introduce the protasis of conditions, *shi* 'if' or 'supposing', may be qualified by another particle *jia* to indicate that the condition is not met: *jia shi* literally means 'falsely supposing' (Harbsmeier 1998: 116ff.).

More generally, the strongest version of the theses of Sapir (1949) and Whorf (1967), according to which thought is not just influenced by what a particular natural language may tend to make salient but is

determined by the characteristics of that language, is equally flawed. Again China provides a test case, where we can examine how Chinese translators in the 16th and 17th centuries got on when faced with the task of translating Western metaphysics. When the Jesuits arrived in China, Chinese converts set about producing Chinese versions of such texts as Aristotle's *Categories*, or rather the Latin version of that work that had been done at Coimbra. Against the expectations that might be generated by the Sapir–Whorf thesis, Wardy (2000) showed conclusively that while, of course, the Chinese translators had to coin new terms and adapt existing ones, they were, in that, no different from, and on the whole not inferior to, the Coimbra Latinists when they struggled with the original Greek text. Native speakers in any natural language, poets included, are, we may be sure, not incapable of expressing exactly what they want to express. Translation of their statements into another language is always difficult and always imperfect: but it is never doomed to *total* failure, as if *nothing* of the original sense could be conveyed in another tongue.

After these unpromising preliminaries it is time to turn to our main agenda. Many quite different postulates of some 'Great Divide' have been proposed, some more clearly defined than others. Among the more sweeping suggestions was Karl Jaspers's (1953; cf. Eisenstadt 1982 and Bellah 2005) notion of an Axial Age when a number of changes took place in China, India, Iran, Israel, and Greece that included the development of complex political organizations, urbanism, agriculture, metal technology, and above all in the critical reflection on the human condition by reference to some transcendent standard. But the first problem here was that approximate simultaneity could only be secured by stretching the Age over several centuries (and not the same ones in each society). The second more important objection was that it glossed over the considerable differences between what occurred in different ancient societies, many of which I have pointed to in my previous discussions.[1]

[1] On the question of transcendence, Runciman (2009: 203) remarked:

> The beliefs of the Greek philosophers are not at all the same as those of the Hebrew prophets with their uniquely disorienting experience of desecration and exile; the *Analects* of Confucius are as different from the Platonic dialogues or the Zoroastrian *Avesta* as they are from the books of Hosea or Amos or Ezekiel or deutero-Isaiah; the Buddhist attitude to political power and those who wield it is no more that of Confucius than that of either the Greek philosophers or the Hebrew prophets; the *varna* model of the four orders is unique to Vedic India; and so on.

Cf. the contributions in Eisenstadt 1986.

To turn to some of the more promising uses of the notion of a Great Divide, it has most often been used in relation to the assumed major breaks with the past that were caused first by the scientific and then the industrial revolutions from the 16th to the 19th centuries in the West. A second important application of the expression has been to the consequences of literacy, when that has been thought to have been similarly transformational in how inquiry and reasoning are conducted. More generally still, in the debates on rationality and irrationality that punctuated the 1960s and early 1970s some argued that rationality is far from universal and diagnosed systematic irrationality on quite a large scale. Earlier still the notion that large segments of humanity suffer from a 'pre-logical mentality' was fashionable for a time in the wake of the influence of the work of Lévy-Bruhl especially. 'Pre-logicality' was identified when it appeared that the normal rules of logic, the Law of Non-Contradiction particularly, were suspended and were substituted by what was dubbed a Law of Participation, which tolerated inconsistencies and incompatibilities.

However, against those views, and in favour of the universalist option, there have been those who have argued that the essential characteristics of human reasoning were laid down during the Pleistocene, and that not much has changed since,[2] including in certain characteristic flaws in our reasoning such as the confirmation bias. At the outset, in my Introduction, I mentioned the work of the evolutionary psychologists Tooby and Cosmides (e.g. 1992), and of those such as Kahneman, Slovic and Tversky (1982) who have discussed judgement under uncertainty. Without necessarily invoking what we owe to the hunter-gatherers of the Pleistocene, other cognitive scientists have homed in on what they would claim to be basic and universal features of human reasoning. According to Humphrey (1976) and others, our capacities in that respect initially developed essentially to answer social needs, to cope with complex social interactions. Mercier and Sperber (2011) analogously put it that persuasion and argument are the key functions of reasoning that every human group needs in

[2] Some evolutionary psychologists, to be sure, have attempted to reconstruct stages in the gradual development of cognitive capacities long before the advent of literacy. Donald (1991), for example, postulates a mimetic ritual stage that antedates a mythical one that itself precedes the stage when cognition could draw on technological support. I fear I judge such ideas to be mere speculations, beyond the reach of either rigorous corroboration or falsification.

order to ensure survival, where, of course, they rejoin Aristotle's observation that humans are indeed essentially social animals.

Obviously the issues are far reaching and of the greatest complexity, and some such as Boyer (2010) would claim that the only way forward is to conduct experimental investigations on both children and adults. Yet some of the basic problems that such research faces have recently been underlined. Two stand out, the possible non-representativeness of those typically investigated, and the artificiality of the situations in which those investigations are carried out.

Henrich, Heine and Norenzayan (2010) drew attention to the first problem, pointing out that the subjects investigated tend to be drawn overwhelmingly from those they labelled WEIRD people, that is Western, Educated, Industrialized, Rich, and Democratic, who in most respects are anything but typical of human beings as a whole.[3] Several commentators have stressed that in the decontextualized situation of the abstract tests devised in psychological laboratories, individuals and groups may perform very differently from, and usually worse than, when they face real-life situations where they have an interest in the outcome and are motivated to arrive at an optimal solution to the problems they are presented with. Of course, the very artificiality of laboratory experimentation can be claimed to be one of its strengths, for extraneous considerations, of values or personal preferences, are factored out. But the possible downside of that artificial situation has been shown by those following up the Wason tests (e.g. Wason and Johnson-Laird 1972), who point out that in the absence of personal involvement, subjects may find it harder, rather than easier, to follow the implications of argument chains.[4]

[3] The experiments carried out by Nisbett and his colleagues (e.g. Nisbett 2003) are exceptional, in that they aim to show radical differences in the ways in which 'Asians' and 'Westerners' think. But the first problem with these studies relates to what those two categories are taken to comprise. Under 'Asians' are included Japanese and Koreans as well as Chinese in the PRC, in Taiwan, and those brought up in the USA, while 'Westerners' covers ancient Greeks and black Americans as well as Europeans. The differences his tests bring to light are more economically explained (as I argued in Lloyd 2007a: ch. 8) as reflecting the divergent attitudes and values of the subjects in question, rather than differences in the ways they think.

[4] In the so-called selection test first devised by Wason (1968, cf. 1966) subjects were presented with four cards each of which, they were told, had either a vowel or a consonant on one side, and either an even or an odd number on the other. They were then given the rule that if the letter is a vowel, the number is even. Presented with four cards where the sides that could be seen showed a vowel, a consonant, an even number, and an odd one respectively, they were asked which cards had to be turned over to disprove the rule. Most

Again I must recognize my personal limitations: being no experimental psychologist myself, I am in no position to comment further on that work. But as a historian and philosopher it seems possible to suggest some clarifications of the overall issues, both in the matter of what there is to explain and on how any explanation might be given traction. My previous explorations of the aims of inquiry in ancient civilizations can be brought to bear first on the differences that literacy made and second on the other main application of the postulated Great Divide, the changes that the Western scientific and industrial revolutions may have introduced.

First, however, it is as well to rehearse some of the background to the debate, which from the early 1960s centred on what came to be called, thanks to a mistranslation of a book by Lévi-Strauss, the 'Savage Mind' (1962/1966). His French title was *La Pensée sauvage*, but that would have been better rendered as 'wild', that is uncultivated, thought. Lévi-Strauss's claim was that what extensive ethnography had revealed was the ways in which what were still called 'primitive' societies exhibited what he termed a 'concrete logic', a logic that used concrete examples, especially myths, to convey sophisticated abstract ideas. Where Lévy-Bruhl had postulated an altogether different logic and a primitive 'mentality' in the societies he read about (he was no field anthropologist, but then no more am I) Lévi-Strauss insisted that concrete logic was well adapted to the expression of complex ideas. However, he still made use of an overall contrast between 'hot' societies and 'cold' ones, a difference of degree if not one of kind.

That thesis was the prime target of Goody's famous (1977) book, which opened with some critical remarks on the series of dichotomies—'savage' and 'civilized' as well as 'hot' and 'cold'—with which Lévi-Strauss still operated, particularly in that they suggested to Goody static entities and did nothing to help account for the transitions between them.[5] To explain those transitions, he focused

saw that the card with the vowel had to be: but only very few recognized that the card with the odd number also had to be, but not the cards with the consonant and the even number; and they even continued to make mistakes when the tests were repeated. The literature that these tests and subsequent variations on them have generated is massive, with no obvious explanation of the phenomenon being generally accepted.

[5] More recently, Descola (2005/2013) has proposed an ambitious fourfold classification of regimes of interiority and physicality, which he labels 'animism', 'totemism', 'analogism',

especially on the role of literacy. One of his main themes was how literacy facilitated not just the recording of facts, but also their more sustained and self-conscious evaluation, and so to the growth of scepticism, and a second important argument of his was the superiority of alphabetic to other modes of writing. His literate versus preliterate contrast has the advantage of being explicitly gradational, but while that, for Goody, was enough for him to insist that his was *not* a Great Divide theory, he still saw the problems as those of the transition between two strongly contrasted set-ups, a Grand Transition theory, one might say, if not a Lévi-Straussian Great Divide one.

Others before and since have joined in, many concentrating not so much on new cognitive tools as on particular sets of concepts. To take just one example of the latter, Gellner (1973, 1985) sketched out a grand plan of the progress of human knowledge where it was the input from, on the one hand, the Judaeo-Christian conception of an omnipotent deity, and on the other, the Greek conviction of the possibility of objective knowledge of the natural world, that played the key role in his conception of the Great Divide. If concepts mark the key differentiating factor, they do not necessarily call for a sociological explanation. But to be sure, such externalist hypotheses have often been proposed, particularly by the likes of Weber (1930, 1948), Merton (1938, 1973), and Butterfield (1949), to account for the phenomenon of the Great Divide, with some focusing on economic or technological factors (such as the rise of capitalism), others on religious ones, especially the supposed influence of the Protestant Ethic.

I shall be commenting briefly on the overall problems in due course, but my first main question relates to the claims that have been made as to the effects of literacy (cf. Havelock 1963; Ong 1982; Olson and Torrance 1991; Olson 1994). What were its consequences? Did they bring about the transition from one side to another of a Great Divide? Did literacy transform the way reasoning and inquiry were conducted? The most obvious effect of literacy is, of course, the simple, indeed crucial, point that records can be kept, and knowledge accumulated and transmitted. Written records of astronomical phenomena

and 'naturalism'. As Taylor points out (2013, against Lloyd 2012a) these are better construed as ideal types than as ontologies. However, the problem of the transitions or shifts in their concrete manifestations in actual historical situations may be thought to remain.

are evidently essential to come to reliable conclusions concerning their regularities and periodicities. But it would surely be rash to suppose that pre-literate groups either in antiquity or today are any less capable of detailed observation, and of carefully evaluating what they had observed, and indeed of persuading others that they knew what they were talking about, than members of literate societies (and we should reflect that in early modern times fully literate individuals constituted only a tiny percentage of any given group as a whole).

Indeed, the evidence that Lévi-Strauss laid such stress on, concerning, for instance, the extremely detailed and complex animal and plant taxonomies in many modern pre-literate societies, gives the lie to anyone who might suppose that without literacy you cannot find brilliant observers and classifiers of the physical world by which we are surrounded. That point still holds, even though we must certainly acknowledge, as Daston and Lunbeck (2011) have recently insisted, that ideas as to what counts as a good and reliable scientific observation have undergone several major shifts in early modern times.

Again there is no reason to doubt the ability of humans everywhere to make well-judged forecasts in practical contexts, in both agriculture and hunting, for example. As we have noted before, their very survival generally depends on that ability. While numeracy is unevenly distributed across societies, and indeed within each and every society, counting and the measuring of quantities figure in everyday transactions just about everywhere.[6] However, the key point is that with literacy not every item of what counted as useful knowledge had either to be preserved by word of mouth or rediscovered.

But Goody had a further claim that, to a degree, scepticism and critical reflection were stimulated by the existence of written records. In oral exchanges claims and counterclaims are evanescent. Once they are pinned down in writing, they can be scrutinized, their weak points analysed, their credibility pondered and assessed.

There is certainly a good deal to this argument, though Goody's further claim that alphabetic writing carried notable advantages over all other kinds is more difficult to evaluate. Although empirical work has been done in an attempt to assess the ease with which different

[6] The thesis proposed by Everett (2005 and 2012) in relation to a supposed total lack of any concept of number and of any ability to count and measure, among the Pirahã, rests on disputed ethnographic evidence.

writing codes can be learnt,[7] I would say the jury is still out. It is always difficult to compensate for our own particular habits (as I discovered when I first had to accustom myself to working with sexagesimal fractions as in Ptolemy's astronomy). Analogously, familiarity with an alphabetic script may lead us to exaggerate the difficulties of other codes. But in two respects especially we need to be careful about literacy as a whole, and that part of Goody's thesis seems to stand in need of greater qualifications than he originally admitted. These concern first the possibility of radical scepticism independently of literacy, and second the negative effects that literacy itself may have.

As to the first point first, there is now abundant documentation available concerning the scepticism expressed in all sorts of contexts in pre-literate groups. Lévi-Strauss (1968: 175ff.) made famous the case of the Kwakiutl Quesalid, who set out with the ambition to show that shamans were all frauds. He learnt or imitated their tricks and performed just as well as they. But the ironical result of this was not what Quesalid had expected. He wanted others to come to the realization that they were being deceived. But in practice his fellow Kwakiutl thought of him as an exceptionally able shaman: willy-nilly he became a shaman himself. That, of course, endorsed that institution, but he started out with fundamental doubts not just about particular shamans, but about the whole lot of them. Doubting individual diviners or whomever is extremely common, but as Evans-Pritchard (1937) showed for the Azande and Keith Thomas (1971) for witchcraft in 16th-century England, that does not undermine the institution as such and may even serve to strengthen it. Quesalid, it seems, was initially more radical.

Are we nowadays more sceptical than our predecessors? Maybe. But we can certainly not say we have rid ourselves of self-deception entirely. Literacy may contribute something to making scepticism more explicit, but scepticism surely does not depend on literacy. The idea that other folk are much more gullible than we are just does not stand up to serious scrutiny.

[7] On the problem of assessing the relative ease or difficulty of learning different scripts, see Rozin, Poritsky and Sotsky 1971; Sakamoto and Makita 1973; Taylor and Taylor 1983; Kennedy 1984; Rayner and Pollatsek 1989. On the supposed overall advantages of alphabetic writing, see e.g. Havelock 1963, 1982, 1986; Ong 1982; Skoyles 1984; Olson and Torrance 1991; Olson 1994.

Then the other qualification I suggested relates to the possible negative effects of literacy (a point already urged by Parry (1985) against Goody's original thesis). Once learning was encapsulated in written texts they naturally often come to be treated as authoritative, as canons in other words, excellent for training the next generation of potential practitioners, but not such good news if the status of the canonical writing veered towards unchallengeability, even approximating to the status of a sacred text. The Mesopotamian scribes consulted a wide range of canons, in astronomy, in medical prognosis, in every department of thought. Over and over again their reports to the kings explain how conscientious they have been or will be in *looking it up*.[8] But the use of *Enūma Anu Enlil* as an authoritative canon could carry disadvantages, if one thinks that some tablets from the Old Babylonian period contemplate eclipses not just at the full moon and the new, but on other, we would say impossible, days of the month (Rochberg 2004: 251). Indeed, the history of science down to the present has been peppered with examples of what Lakatos (1978) dubbed degenerating programmes of research, when initially promising lines of inquiry ossified. The danger in antiquity was always that a compendium would be treated not just as a useful database, but as having set down the last word on the problems.

If, as we have also argued in previous chapters, there is plenty of evidence from pre-literate societies not just of scepticism but also of observation, of the use of trial-and-error procedures, in what circumstances could it be claimed that literacy transformed the ways in which reasoning was conducted? I mentioned in Chapter 2 some of the evidence that shows that debate and persuasion are often thought of as highly prized skills in societies that lack writing. One key issue here is whether faults in reasoning are more liable to be committed and to go unnoticed in the absence of literacy.

As remarked already, the idea that the basic logical rules of Non-Contradiction and Excluded Middle were flouted in pre-literate

[8] Thus asked about predicting eclipses, one scribe answered, 'We will look it up—and I shall send the king, my lord, a tablet dealing with the halo of the moon' (*State Archives of Assyria* X Tablet 71, Parpola 1993: 53). Concerning auspicious days, another wrote: 'As the king, my lord, knows, an exorcist has to avoid reciting a "hand-lifting" prayer on an evil day: therefore I shall now look up, collect and copy numerous, 20 to 30, canonical and non-canonical tablets, but perform the prayers only tomorrow evening and on the night of the 15th day' (*State Archives of Assyria* X Tablet 240, Parpola 1993: 191).

societies was one of the main planks in Lévy-Bruhl's postulate of a 'pre-logical' mentality. Yet the attempts that were made to diagnose either some basic lack of logical skills or indeed a reliance on some other, non-standard logic (what he called the logic of participation) now all seem like desperate attempts to drive a decisive wedge between us, civilized people, and the rest of the world.[9] Yet as already noted, empirical studies have shown that humans in modern industrial societies, even trained statisticians, make some of the same types of mistakes in inference as those that used to be thought characteristic of primitive thought, let alone of a pre-logical mentality. Tversky and Kahneman (1982) in particular have shown that humans everywhere exhibit similar biases, for example in predicting the outcome that best suits the immediate data with insufficient regard for prior probabilities.[10]

The chief respect in which the conduct of reasoning *does* differ from one group to another is where explicit rules are available, formal logic in other words, to which appeal can be made to determine validity or invalidity. Yet the trouble with formal logic is that its applicability to informal modes of reasoning is limited. It analyses the relations between well-formed formulae, wffs for short, but they are not common in ordinary discourse. Thus Luria in a famous study of reasoning among the Uzbeks concluded that they were seriously bad logicians. In one set of exchanges,[11] he told his informants that in the Far North where there is snow, all bears are white, and also that Novaya Zemlya is in the Far North and there is always snow there. He then asked what colour the bears in Novaya Zemlya were. To which one of his respondents replied, 'I don't know.' Another put it: 'You've seen them: you know. I haven't seen them, so how could I say?' Yet instead of concluding that his respondents were weak at logic, the exchange

[9] Non-standard logics have been claimed to be useful in certain contexts in computer programming. But they are no more useful than the standard version if we seek to capture the characteristics of informal reasoning, the key to which generally lies (as I go on to say) in pragmatics.

[10] Having identified how errors due to 'insensitivity to prior probability of outcomes', 'insensitivity to sample size', and 'misconceptions of chance' are widespread, Tversky and Kahneman (1982: 18) noted:

> The reliance on heuristics and the prevalence of biases are not restricted to laymen. Experienced researchers are also prone to the same biases—when they think intuitively. For example, the tendency to predict the outcome that best represents the data, with insufficient regard for prior probability, has been observed in the intuitive judgements of individuals who have had extensive training in statistics.

[11] Luria 1976: 108–9, 112.

might have, indeed should have, suggested to him that they were excellent at pragmatics. By asking the question 'What colour are the bears in Novaya Zemlya?' Luria had implicitly cast doubt on the earlier information that in the Far North all bears are white.

However, in one respect at least the existence of formal rules of argument obviously does make a difference. This is, as I said, where in argument an appeal can be made to such a rule in order to suggest that an inference is faulty. Once what I have called explicit concepts of logical categories exist they can be invoked to call attention to such a problem. Diagnosing a breach in the Law of Excluded Middle in the flow of ordinary conversation may be trickier than the handbooks on logic might seem to suggest, but once the possibility of such a diagnosis is available it may be used to identify where reasoning may go wrong. As I have just noted, well-formed formulae are not the common currency of argumentative exchanges anywhere in the world outside classrooms teaching formal logic. Nevertheless, some sense of possible flaws in reasoning need not depend on fully fledged formal rules, but merely on a hunch that some inconsistency has been committed. Now notions of inconsistency themselves certainly pre-exist the first formal logic:[12] yet with formal logic the criticism of another's reasoning is facilitated (whether justly or not) by the existence of named and labelled logical rules.

Obviously challenges to another's point of view may take many different forms: the accusation may be of incompetence, of lack of experience, of corruption, of deception—none of which can be said to be limited to situations where participants command rules of formal logic. But the explicit forging of categories relating to logical inconsistency marks a particular new brand of challenge. So at this point the quality of the argumentative exchanges may be affected by the degree of self-consciousness with which logical rules are recognized and formulated. Let me recall the passage from Aristotle (*On the Heavens* 294b6ff.) that I cited in Chapter 4, when he observed that we are all in the habit of directing our arguments against our opponent rather than

[12] There was no formal logic of the Aristotelian type in ancient China. But they had a clear notion of inconsistency and the need to avoid it. They used the term *bei* to convey this and exemplified with the anecdote of a salesman who claimed simultaneously that his lances could penetrate anything, but also that his shields could withstand penetration by anything. 'Lances and shields' (*maodun*) was one way of conveying the notion of inconsistency metonymically.

at the subject matter in hand. It seems that he was well aware that probing a set of ideas for inconsistency or other logical weaknesses is more often deployed in relation to the views of your opponent in argument, less often in relation to your own views.

So let me now sum up the key issues in this brief discussion of the consequences of literacy from the point of view of the development of reasoning. First, writing evidently enables the creation of records that are essential for such activities as the study of the periodicity of eclipses and those of planetary movements, as well as important in many other areas such as medicine, law, and historiography. Hard-won data can be diffused and exploited far more easily once written down, though what is thus diffused will not just be such data but also what passes as conventional wisdom, whether well grounded or not. Moreover, the extent of the diffusion is limited to those who have access to the texts in question, which may be an exclusive elite group and certainly not cover all those who have some measure of literacy in the society concerned.

Then once writings exist that are believed to encapsulate the whole of knowledge in a given field—when they are turned into canons, in other words—they can be very useful for the training of the next generation of scholars, but have the effect of inhibiting innovation and the critical assessment of that knowledge. Writing further facilitates the drawing up of rules governing reasoning and argument themselves, eventually the production of formal logic, where again the impact on practice may be complex. On the one hand, the formulation of such rules allows them to be appealed to in interpersonal exchanges and as the wherewithal to check the validity of any sequence of arguments. On the other, those who have mastered such rules are certainly not immune to making mistakes in inference, which may not be at all easy to diagnose with any confidence, given that most reasoning is informal and depends on pragmatic considerations that are not encapsulated in well-formed formulae. The evidence shows that it is not just those who lack writing but also literate members of so-called advanced modern societies, even those who have been schooled in logic, who are subject to such logical errors.

The next big question that confronts us is to consider the phenomena that tend to be discussed under the very general and possibly confusing rubric of the 'scientific revolution', where there has been and continues to be considerable disagreement both about what that

consisted in, when and why the crucial events occurred, and what factors can be invoked to explain their occurrence. While most identify European developments in the 16th and 17th centuries, the work of Copernicus, Vesalius, Galileo, Kepler, Harvey, Newton, especially, as the key, others have suggested that important breakthroughs were made much earlier, in the 12th and 13th centuries, for instance (a favourite theme with Alistair Crombie, 1994). A further topic of controversy relates to the apparent lack of analogous changes in other civilizations, generating the so-called Needham question in particular, as to why there was no scientific revolution in China, when the Chinese had been so far in advance of the West in many areas of technology until the 16th century. Now I would judge that question to be ill formed: one cannot hope to explain why such complex phenomena did *not* occur with the same precision with which on some occasions, at least, we can identify the causes at work in the cases of actual events. In that sense non-events may be the subject of endless speculation. Nevertheless, I shall return to the issues at the end in a bid to clarify what is at stake.

Science, as we know it today, is certainly a global phenomenon. But we must be clear what it means to say that. Needham's own assumptions, for instance, which were not tied to his discussion of the 'Needham question' relating to China, combined two principles between which there is some tension. On the one hand, he asserted (e.g. Needham 1978: 110) that just about everyone would agree that there is 'only one unitary science of nature'—which consistently led him to look for and to find continuities between ancient and modern ideas and practices. On the other, he warned against treating ancient endeavours as 'failed prototypes' of modern science, saying that we 'must get inside the minds' of ancient researchers to see why they came to the conclusions they did. While the second principle is one I would endorse (although he there expressed it in overly mentalistic terms), I dissent from the first insofar as it may imply that nature is a single unproblematic given, whose secrets are just there, as it were, to be uncovered. The thrust of my argument in this book is to make due allowance for the multidimensionality of the phenomena and to stress the legitimate divergent understandings of what is there to be investigated.

So what it is to say that science is now a global phenomenon is simply to point to the fact that while there are still considerable

divergences in the interests, agenda, techniques, and instrumentation in different laboratories across the world, over a wide range there is convergence in the endeavours undertaken and the methods used to pursue them. Biologists, physicists, cosmologists everywhere share a broad agreement on many of the key issues they face, they share similar training, and are very well aware of the work of colleagues in other research centres. Nor does their particular nationality make an appreciable difference. Even though the natural languages of Japanese, Chinese, Indian, Russian, German, American, British scientists differ, they have little difficulty understanding one another's work. In the case of mathematicians, their findings are communicated using the lingua franca of mathematics itself.

Yet we should be careful on two scores. First, there is still pluralism in the sense that different researchers concentrate on different aspects of their subject and sometimes take very different approaches to the same current problems in those subjects. Second, and more importantly, it is not as if definitive results are just around the corner, that all the key ontological issues have been settled and that the way ahead is clear. One has only to think of the ongoing problems in fundamental particle physics, in reconciling quantum mechanics and relativity, in the search for the Grand Unified Theory (GUT), in the exploration of black holes and antimatter, and in a host of issues in genetics since the discovery of DNA, to realize that. At the level of pedagogy, science tends to present a united front. At the cutting edge, many fundamental questions, including ontological ones, remain and may even appear intractable: indeed, it would not be a cutting edge if that were not the case. Increasing knowledge brings increasing awareness of what still eludes understanding, and this may include issues that are analogous to those I have been concerned with in previous chapters, about what is there to be investigated, how to go about its investigation and about what would count as a satisfactory resolution to the problems. We may have better grounds than our predecessors for supposing that some of what we nowadays take for granted is secure, but dogmatism about results is as misplaced today as it has always been.

So justice needs to be done both to where modern science is continuous with, and where it diverges from, earlier investigations, and to what those similarities and differences can tell us about human cognitive capacities and their realization.

A first crucial point stems from the very global nature of science that I have just drawn attention to. Given the appropriate training, a young person anywhere in the world can join the undertaking. They need skill and talent, and no doubt have to have interpersonal abilities as well: but their entry into the ranks of the scientific elite does not depend on nationality, ethnicity, let alone gender. Thus far we have no need to postulate a particular special capacity for reasoning of a distinctive kind in order to make room for even the most advanced science.

Yet as I have repeatedly underlined in the preceding chapters, science as we know it today does differ profoundly and substantially from any ancient investigations. Not only are the concepts used distinctively modern—the ancients had no genes, no idea of pure chemical elements, let alone quantum mechanics, as I said—but also their ontological assumptions differ in the ways I have discussed. Moreover, the manner and the contexts in which the phenomena are studied differ. Thus several of the 'styles' identified by Hacking in his (1992) paper, and by him and others since, do not antedate the 17th century. The notion of a statistical analysis of the data depends on the development of the concept of probability which happened in quite specific circumstances in that century (Hacking 1975). More strikingly still, the set of phenomena covered by the rubric of 'laboratory life' is an even more recent development, as is the importance of computer modelling, which Hacking now rightly stresses should be added to the list of distinctive modern 'styles of thinking & doing' (where he now insists on copyrighting the ampersand; Hacking 2012). The question is: How far do these obvious differences lead us to qualify or even retract any universalist claims concerning human reasoning?

We have seen that ancient inquiries themselves differed, reflecting a great variety of factors, not that the investigations of any given individual in any tradition were entirely determined by the circumstances in which they worked, though we have seen a good deal of variation in how they negotiated the room for manoeuvre they needed to innovate. I have drawn attention to the importance of political ideology, of the existence of institutions supporting learning and research, of the values of the group or the society in question, and I have further insisted on the differences in ideas about the phenomena worth investigating. Those differences reflect values, to be sure,

but also the opportunities the phenomena themselves present, where their possible multidimensionality becomes important. A similar amalgam of factors continued, of course, to operate when 'science' as we know it today eventually took off, for that depended, for sure, on new ambitions, new institutions, new values, and new perceptions of the possibilities of investigation.

Yet while the social and intellectual frameworks in which inquiry into the physical world have changed—and continue to do so—it can nevertheless be argued that the faculty, reason itself, exhibited in inference, judgement, persuasion, and so on, did not. The argument would be that the potentiality to conduct investigations in different modes, styles, and with different interests and agenda does not vary, at least as the potentiality it is, even while the actualization of some style rather than another is a matter where cultural, social, and even personal differences count.

The analogy with language skills is suggestive, both for the similarities and the differences it reveals. On the one hand, normal human beings anywhere in the world and at any time since the appearance of *homo sapiens*, at least, have the capacity to learn a language: they acquire a competence from an innate capacity,[13] even though performances still differ. On the other hand, the particular language that any individual learns reflects his or her environment, and there are some parts of the world where most individuals acquire more than one language, in some instances quite a number. Amazonia and Papua New Guinea both provide excellent evidence to that point, including evidence that relates to the practice of what is called linguistic exogamy.[14]

The similarities with the practices of science are again first a matter of the potentiality to acquire the necessary skills to join the team, and second that here too the innate capacity can be distinguished from performance. Yet unlike the language case, it is not necessary that an individual should participate in what passes as the scientific or exploratory endeavours of his or her group. Where language is essential for the survival of the group and of the individuals within it, and

[13] Whether all natural languages exhibit the same deep structure, a Universal Grammar, or not, remains highly controversial. With Chomsky 2006 and Crain 2012, compare Dor and Jablonka 2001 and Evans and Levinson 2009.

[14] See, for example, Hugh-Jones 1979: 17ff. and Jackson 1983 on linguistic exogamy in Northwest Amazonia. On multilingualism in general, see Wardhaugh 2010.

so too, as I remarked, is reason as manifest in inference, persuasion, and debate, individuals do not have to engage in self-conscious, deliberate investigations of the physical world, in research in other words. Of course, as I argued before, observational skills and the use of trial-and-error procedures to get further knowledge are widespread if not universal. But it is another step to turn an innate curiosity into the driving force for sustained investigations.

So I return to the Needham question relating to China to see what light we can throw on the issues, in particular by endeavouring to distinguish between those problems where we can hope to make some progress by way of historical studies and those where we are bound to face an impasse. To pose the question of why China did not experience the Western scientific revolution of the 16th and 17th centuries falls into the latter category. What we mean by those 'revolutions' is a highly complex set of phenomena, masked by the fact that the term 'revolution' itself tends to imply that the changes in question were well-defined historical events. To understand what actually occurred and why has exercised historians for decades. Not only are the explananda complex, but simple answers to those questions are never going to work. What influenced or facilitated the researches of each of those pioneers I mentioned before, Copernicus, Vesalius, Galileo, Kepler, Harvey, or Newton, let alone a host of other less iconic figures—and were those enabling or inhibiting factors the same in every case? The work of any given individual, and not just the great geniuses, is always going to elude hard-edged explanation. In each case the problem situations they faced differed, as did their personal ambitions and the specific relations with the institutions and the patrons with which they had to deal. To suggest that a single stimulus, or even a clearly defined set of them,[15] is at the root of the specific work that each and every one of them undertook, including the specific

[15] Huff's (2011) re-examination of the problem does allow for considerable complexity, but tends to run together postulated causes and effects. He finds a new scientific attitude, and a new curiosity, in the West in the 16th and 17th centuries, claiming, perhaps rather too sweepingly, that they are absent from Mughal India, the Ottoman Empire, and Ming and Qing China. But those are, of course, the explananda, as also is the concentration on experimentation. The explanatory factors that he invokes (2011: 302) include the facilitating effects of religion (as in the familiar thesis of the Protestant Ethic), the crystallization of a public sphere (where the contrast is with exclusive elite groups in non-Western societies), and the rising rate of literacy (though surprisingly he makes no mention of Goody in this context). The contrast between his discussion and the painstaking explorations that Sivin undertook of the shifts and breakthroughs that can

'breakthroughs' for which they appear responsible, is to oversimplify grotesquely. It follows that hunting for missing stimuli in China turns into a wild-goose chase.

The proper studies that a historian can undertake relate to what actually did happen, and the complex factors at work in each situation. We can investigate the elements that favoured certain new demarches in Athens in the 5th and 4th centuries BCE and in Alexandria in the 4th and 3rd, as we can in China in the Warring States, under the Han and later. What we can do is to explore (as we did in Chapter 3) why heliocentricity when it was proposed by Aristarchus was generally rejected—the arguments that weighed with Ptolemy when he did so—or again why Shen Gua had only limited success with his astronomical reforms.[16] And so similarly for the achievements and the obstacles encountered in 10th-century Cordoba, in Mughal India or wherever, where specialists in each of those fields have to come to grips with the complex specificities of each situation, always guarding against the temptations of anachronism.

The actual trajectory of the development of new ideals for inquiry differs in different societies and at different times. To expect all of them in pre-modern times to conform to a single pattern, the one we are familiar with in the West, is to indulge in teleological speculation. We have seen that in antiquity it took a certain combination of factors to promote the exploration of a new style of demonstration, securing incontrovertibility, in ancient Greece. Again rules of debate and methods of discovery became the subject of self-conscious analysis in several ancient civilizations, again reflecting the complex circumstances that prevailed in each—not that that resulted in the same analysis in each case. The pace of change certainly accelerated though not uniformly in every field of investigation from the 16th century, reflecting further changes in aims, ideals, institutions. The rise of rampant materialism is now, for sure, a global phenomenon, though one that has rebounded on the image of science itself, reminding us that it is an instrument that can be, but is not necessarily, used for the

actually be ascribed to China in the early modern period (as I mention in the next note) is striking.

16 Sivin (1995a: VII), in a famous paper, even suggested that the history of Chinese thought was punctuated by a series of major transformations: so why, he asked, did the Scientific Revolution not take place in China—or didn't it?

'benefit of all under heaven', as the ancient Chinese put it. We can, of course, see immeasurably more with the tools that are now available, optical and radio telescopes, microscopes and the like, where analysing the data with computers adds enormously to their usefulness. But what we observe always has to be processed and interpreted in the light of assumptions, hypotheses, conjectures, even when we use computer modelling again to help in that work. We cannot escape our assumptions, though we can be critical of them, just as our predecessors did not escape theirs—and yet many of them too saw the need to be self-critical.

In today's science we do not bring into existence a new faculty, even when we develop a new style of reasoning. The same underlying capacity, more or less aware of its fallibilities, more or less trained, more or less 'domesticated', is in play throughout. Certainly the *practice* of certain skills, in navigation, in playing the piano, and indeed in reading and writing themselves, has been shown to be accompanied by neurological changes that show up in fMRI scans and the like (Changeux 1985; Jablonka and Lamb forthcoming). In that sense it is obvious that the cognitive activities that humans engage in are unevenly distributed across individuals and are capable of increasing or indeed atrophying. But that is no reason to suppose that only some humans, but not others, possess the *potentiality* to develop the corresponding cognitive capacities. On that score there is no need to postulate some Great Divide within human populations as a whole.

No doubt some may think it excessively provocative to imply that from the point of view of the faculty, that is that potentiality, there is no fundamental difference between a 21st-century physicist, an ancient *ţupšarru*, and a hunter-gatherer in the Pleistocene. But by that claim I mean to draw attention to the commonalities I have been exploring. It is to say that all engage in processing whatever input they receive, whether from observation, aided or unaided, or from the communications of other humans: all exhibit curiosity, to a greater or lesser degree; all engage in persuasion and social exchange. At the same time the ways all those capabilities were and are used certainly do differ, reflecting the tools available (including the technology of writing) the styles and modes of inquiry we can distinguish, the ideals and motives that drive investigations, the institutions within which they are conducted, and especially the values that underpin them, as

we saw already in ancient civilizations. Nor should we forget that different aspects of the appearances may engage the attention of different individuals and groups, for the investiganda are never straightforwardly given.

It is time now to draw the threads of my argument together and to set out as clearly as possible the elements of commonality, and of distinctiveness, in the use of reason and its role in the development of science. (1) According to an antiquated positivist view, the contrasts between primitives and moderns were radical. The members of non-literate societies were thought to suffer from various types of deficiency: according to some they manifest a pre-logical mentality altogether different from our own, and they were certainly incapable of science as we know it. (2) Ancient societies were recognized to have literate minorities, but were generally thought to have been held back by political ideologies ('oriental despotism') or other institutional or intellectual shortcomings. (3) A partial exception was made for some ancient Greeks, to whom was attributed at least the beginnings of scientific endeavours comparable to our own. (4) However, it was only after the upheavals of the 16th and 17th centuries in the West that one branch of scientific inquiry after another began to take off in exponential growth.

I have challenged all four sets of assumptions in different ways in the studies in this book. (1) Ethnography clearly shows conclusively that the members of pre-literate societies can be, indeed have to be, excellent observers; they exhibit curiosity and scepticism and evidently engage in trial-and-error procedures. They have complex understanding of their environment and of their own place in it. Neither the Western notion of 'nature' nor its antonym 'culture' fit at all easily on to those understandings, as the explorations of Viveiros de Castro (1998, 2009) and of Descola (2005/2013) have shown, further strengthening the point that the idea that there is a single nature out there for all of us to explain is a chimera.[17] Moreover, we

[17] Viveiros de Castro (1998, 2009) has put the point most forcefully. In contrast to the Western assumption that nature is universal and cultures differ, the Amazonian peoples he studies (and not just them) rather assume that culture is what all beings share, but the natures they are confronted with differ. So not mononaturalism and multiculturalism, but multinaturalism and monoculturalism. I would agree with the thrust of this argument, though would note that it still stays in thrall to the twin concepts of 'nature' and 'culture'. My (cf. 2007a: ch. 7) view would be that any use, however revisionary, of that dichotomy is liable to mislead. To avoid the traps that the use of the historically loaded concept of

can document plenty of cases, from non-literate societies, of an extensive and sophisticated interest in debate and argument (Chapter 2). If we accept that one, and maybe the most important, function of human reasoning is social, we can certainly see that at work in present-day societies, and many would suppose that such interests and abilities were present already amongst the earliest members of the species *homo sapiens*.

As for (2) and (3), we must, in my view, recognize (a) the actual variety of investigations for which we have evidence not just from Greece, but also from Mesopotamia, India, and China. And (b) the legitimacy of alternative views both on what there is to investigate and on how to investigate it. We have seen reason to resist the idea that there was just a single route that early explorations of the physical environment had to take: and we may now add that we should recognize a pluralism also in modern science especially where it too faces intractable problems.

(4) That is not to deny that since the 'Scientific Revolution' our knowledge of physics, astronomy, biology, and many other fields has indeed been transformed. For that to happen, as I have remarked, new concepts had to be made explicit, new tools and instruments devised, new institutions established, reflecting new values, aims, and programmes. Yet alongside the revolutionary elements, I have emphasized what links modern endeavours to past ones, including in the cognitive capacities brought to bear. It was not the case that modern scientists were the first to carry out careful observations (see point 1): experiment is more systematic and controlled than trial-and-error procedures but builds on them; ideas about what counts as a conclusive proof have been taken up as well as modified views that were already expressed by some Greeks. And although they contrasted axiomatic-deductive demonstration with other modes of persuasion, it was in effect, as Aristotle saw, the most persuasive form of persuasion of all, and persuasion has good claims to be a fundamental

nature implies, I would argue that it is preferable to speak rather of the multidimensionality of the phenomena, allowing that the 'phenomena' may differ for different observers. Latour's (2012) analysis of multiple, overlapping and interdependent, modes of existence is more radical still, but shares with the work of Viveiros de Castro the ambition to challenge the monopoly that the ideology of science currently still enjoys in the West. Doubts about the ethnographic basis for the new ontological turn in social anthropology have, however, been recently expressed by Turner (2009).

function of human reasoning. In each case, as the cognitive capacity came to be the subject of self-conscious reflection and analysis, that served to stimulate its use—and conversely the increasing exploitation of the capacity leads to further reflection and analysis in a cumulative feedback effect.

But if there are this diversity and pluralism in the actual trajectory that investigations took in different ancient societies, how is that to be accounted for? The constraints under which the investigators operated surely provide at least part of the answer, though that is not to suggest that their results were simply determined by the circumstances in which they worked. However, it seems possible both to identify certain commonalities in the *types* of constraint in play, and differences in the actual *manifestations* of those constraints at particular historical junctures.

Obviously those who engage in scientific investigations of any kind work and have always worked under political, economic, institutional, professional, technological, and ideological pressures. Our study of antiquity shows that no one political regime consistently delivered positive results. What democracy sometimes secured was open debate, freedom of speech, in other words, within limits, if you could get away with it. But autocracies could be more effective in sponsoring research (and not just into weaponry), which, however, they could also stifle. Institutional or 'professional' structures are needed to help establish a discipline, but again as we have seen they could and did and still do control the agenda and inhibit innovation. To make their reputation, researchers have always to come to terms with a target audience, whether in face-to-face encounters, or through writings, whether that be some patron, or the ruler, or the political authorities, or a peer group or a bunch of laypersons. There was no uniformity in the ancient world—nor is there today—in the ways in which in different societies different individuals or groups, working in different more or less well-defined fields of inquiry, at different periods, have responded to all those pressures.

At the same time we should not lose sight of a further point, that they all, to a greater or lesser extent, had to recognize further constraints from the side of what it was they were investigating. As I put it before, an eclipse would not occur just because some ancient *ṭupšarru* said it would. A patient would not get better just because some ancient healer said that he or she would. We are now more confident

in our experts, and in most of the knowledge they deliver: but they are certainly neither infallible nor omniscient, on fundamental ontological questions in particular, even though they have more powerful means to justify their claims to expertise than their ancient counterparts. Pioneering science has become, if anything, more difficult for a lay individual to evaluate, but if appeal is then made to the judgements of a peer group, those peers have been shown to have sometimes made mistakes in their initial evaluation, as the original reception of the discoveries of both DNA and plate tectonics in the late 20th century illustrates. It is a delusion to suppose that even the best-established elements of science are immune to correction or even that we have sure-fire means of distinguishing the innovations that will persist from those that will shortly just seem unfortunate fashions.

So just as I concluded for antiquity at the end of the last chapter, a balance needs to be struck. The specifically modern manifestations of the ambition to inquire should not lead us to underestimate the commonalities with much earlier endeavours. Of course, nowadays the more or less unassailable popular prestige of science allows the researcher to retreat a step or two in the face of temporary difficulties without that threatening the very foundation of the whole endeavour. That was much more difficult at the very outset of sustained inquiry, where so much more energy had to be devoted to establishing the possibility of the enterprise—some enterprise, your idea of that enterprise—in the first place. The capacities to investigate, to question, to try things out, to argue with your fellows, are universal across all human populations. But their manifestations, as I put it, have differed and continued to differ. The first self-conscious analysis of those faculties was the achievement of ancient literate societies, not that the paths they took in their explorations were uniform.

It is in that context that their diverging views on ideals, methods, programmes, about how to investigate, what there is to investigate, and what it is good for, have lessons for us still today. Their ancient history was anything but predictable: nor can we be confident that we can foresee what our future holds. But at least we can ponder their experiences and use them, hopefully, to further our own self-understanding. In that respect I would claim that this ancient history of ideals continues to be relevant to our own predicaments today, and it is in that spirit that I have undertaken this investigation.

GLOSSARY OF CHINESE TERMS

誖	*Bei*	inconsistent
大數	*Da shu*	'general numerical regularities'
道	*Dao*	the Way
府	*Fu*	yang visceral systems of function
訶難	*He nan*	'stumping', causing difficulties
假使	*Jia shi*	'falsely supposing'
經	*Jing*	tracts
里	*Li*	'league' (measure of length)
脈	*Mai/mo*	vessels
矛盾	*Maodun*	'lances and shields' (metonym for inconsistency)
難解	*Nan jie*	quizzing
氣	*Qi*	breath/energy
俗醫	*Su yi*	common doctors
天下	*Tian xia*	'all under heaven'
無為	*Wu wei*	'no ado'
五行	*Wu xing*	five phases
行	*Xing*	going, activity
議	*Yi*	records
陰陽	*Yin yang*	negative and positive principles
曰	*Yue*	says, means
藏	*Zang*	yin visceral systems of function
則	*Ze*	so, then
眾醫	*Zhong yi*	the mass of ordinary doctors

NOTES ON EDITIONS

Greek and Latin texts are generally cited according to Oxford Classical Texts or Teubner editions or to those specified in the fourth edition of *The Oxford Classical Dictionary*, ed. S. Hornblower and A. Spawforth (Oxford 2012). Where there is any doubt, I name the editor of the text I cite. Thus Archimedes is cited according to the edition of J. L. Heiberg, revised by E. S. Stamatis, Euclid also by the edition of Heiberg et al., revised by Stamatis, Ptolemy's *Syntaxis* according to the edition by Heiberg, his *Tetrabiblos* according to the edition by Hübner, and his *Planetary Hypotheses* according to *Opera Astronomica Minora* vol 3 (J. L. Heiberg, Leipzig 1907). For Greek and Latin medical texts, I use the *Corpus Medicorum Graecorum* (*CMG*) and *Corpus Medicorum Latinorum* (*CML*) editions for preference. For those Hippocratic texts not included in *CMG*, I use E. Littré, *Oeuvres complètes d'Hippocrate*, 10 vols. Paris 1839–61, cited as L followed by the volume number and page, though for the treatise *On the Sacred Disease* I use the *Ars Medica* edition by Grensemann (Berlin 1968). For the medical history in *Anonymus Londinensis*, I use the edition by D. Manetti (Berlin 2011). For Galen's works not included in *CMG*, I use the edition by C. G. Kühn, Leipzig, 1819–33, cited as K followed by volume number and page, except for texts included in the Teubner *Scripta Minora* editions. For the later books of *On Anatomical Procedures*, extant only in Arabic translation, I cite Duckworth 1962.

Chinese texts are generally cited according to the standard editions, for example from the Harvard–Yenching Institute series, the University of Hong Kong Institute of Chinese Studies series, and the *Zhonghua shuju* dynastic histories. However, for *Huainanzi*, I use the edition by Liu Wendian (Shanghai 1923). For the *Huangdi neijing lingshu*, I use the edition by Ren Yingqiu (Beijing 1986). For *Lüshi chunqiu*, I use the edition of Chen Qiyou (Shanghai 1984) but I adopt the chapter subdivisions in Knoblock and Riegel (Stanford 2000) and for *Xunzi*, I adopt the chapter subdivisions in Knoblock (Stanford 1988–94). For the *Zhoubi suanjing*, the *Jiuzhang suanshu (Nine*

Chapters of Mathematical Procedures), and the commentary by Liu Hui on the latter, I use the edition of Qian Baocong, *Suanjing shishu* (Beijing 1963).

All modern works are cited by author's name and year of publication. Full details are to be found in the Bibliography that follows.

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